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Philosophy of Science

VOL. 3 July, 1936 NO. 3

The Inductive Argument for an External World[1]

BY

EVERETT J. NELSON

METAPHYSICAL problems may be solved by the methods of inference employed in the empirical sciences. So we are told by many realists and pragmatists, among whom may be mentioned Professors J. B. Pratt, William Savery, and Donald Williams. Mr. Williams and Mr. Pratt have argued for the use of inductive methods in establishing the existence of an external world. Mr. Savery has asserted that all philosophical inference as to matter of fact is inductive.[2] This naturalistic attitude is by no means unusual—in fact it is so common in this age in which the speculations of scientists as well as their achievements have captivated the popular imagination, that even philosophers have for the most part failed to examine its validity and presuppositions. It is the purpose of this paper to present such an examination, not of the philosophic employment of inductive methods in general, but in regard to one specific problem; namely, that of

[1] Read in condensed form at the meeting of the Pacific Division of the American Philosophical Association, Stanford University, December, 1935.

[2] J. B. Pratt, "Logical Positivism and Professor Lewis," *Journal of Philosophy*, XXXI, No. 26, Dec. 20, 1934, pp. 701–710. William Savery, "Chance and Cosmogony," *The Philosophical Review*, XLI, No. 3, March, 1932, pp. 147–179. Donald Williams, "The Argument for Realism," *The Monist*, July, 1934.

the existence of an external world. What I purpose then is to present and evaluate the view that the existence of an external world may be established by inductive methods as employed in the empirical sciences.

1. *External World Defined.* By an external world I mean any particular or system of particulars, of whatever nature—mental, material, *x*-nature—which is not immediately experiencible. Such a world would not include sense-data not sensed, if there be any, because they would be items of possible experience. If a representative or causal theory of perception be true, then scientific objects such as electrons, fields of force, ether, etc., would be in the external world. Similarly, if Berkeley be right, his God is part or all of the external world. Since an external world would not enter my experience directly, I could know that it exists only by inference from data I immediately experience, or by deduction from premises otherwise obtained.

2. *Problem Stated.* The problem before us then is this: Is it possible to construct a valid inductive argument which concludes from phenomenal evidence or data of experience to the existence of an external world? The question is not simply whether there is sufficient evidence to justify belief in such a world, but rather whether phenomenal evidence can render to such a belief any finite probability, however small.

The inductive argument for an external world takes this form: If there be an external world, then we can account for the orderliness of phenomena. Therefore, such a world exists. Thus, that such a world exists is presented as an hypothesis for verification by experience, which, it is urged, may be rendered more and more probable by the positive verification of its consequences or of predictions based upon it, just as in the case of a scientific hypothesis. It is the alleged validity of this argument which I want to examine.

3. *Some Objections Answered.* Before proceeding I want to get rid of certain common fallacious arguments. One of these is simply to ask the question, "How explain the success of science if there are no atoms, molecules, fields of force, etc.?" Another is, "How explain the sequences of sense-data unless an external

world be assumed?" I admit that these are very difficult questions, but the asking of a difficult question is surely quite a different thing from *showing* that the question cannot be satisfactorily answered except upon a certain assumption. Hence, though I might be unable to answer these questions, I am not for that reason in any way obligated to believe in the existence of such entities. As a matter of fact, I think that the positivistic answer contains no logical or experimental difficulties so far as science is concerned. But even if this be false we must remember that no one has ever shown with any probability that there *is* any true explanation of phenomenal sequences. And if you cannot prove that there probably is an explanation, it is difficult to see how you can show that a given particular explanation is probably correct.

Moreover it is important to see that the positing of an external world does not at one stroke remove all the difficulties in the validation of empirical inference. The problem is not so simple as all that. For example, it would not be hard to show that the following question would tax the ingenuity of most of us, "How justify induction even with an external world?" As examples of the difficulties that would have to be met in answering this question I mention only two: (1) The presentation of some positive reason to believe that a genuinely causal or generative relation is the effective connection between the phenomenal and external worlds. Functional relations or correlations are not sufficient, because the explanatory purpose of an external world is not to establish a correlation between phenomena and events in it, but to account for, or form a ground for, phenomena. Empirical correlations do not *account* for the orders of phenomenal events, for the law of correlation simply expresses these orders. The supposition of A to account for B is useless unless the relation between A and B be such that the existence or nature of A would really be a sufficient condition of the existence or nature of B, and by "sufficient" I mean metaphysically so, and not simply causal in Hume's sense. Hence, even though we may not know the properties constitutive of such a relation, we do know this much, that the type of connection between

phenomena and events in an external world must be radically different from functional or ordering relations of the type the empirical sciences discover between empirical events. Moreover, if the external world be not of the nature of the data of experience, it is gratuitous to assume that a connection of the type that could hold between data of experience could hold between such data and events in the external world.

(2) The second difficulty I wish to mention is the establishment of a view which will insure a stable order in the external world itself. Since the laws of physics that are supposed by some to hold between the elements in the external world are nothing but functional relations, the order in such an external world is in exactly the same need of explanation and stabilization as the order of phenomena. To satisfy a philosopher's demand for understanding, one realm of contingencies as the ground for another will never suffice.

I think these points show that the belief in the existence of an external world may be legitimately questioned, and that even if there be such a world, considerable theory and argument are necessary to render it a satisfactory explanation of experience.

4. *Problem of Induction.* Since the argument in question is inductive, I must indicate briefly before evaluating it the present state of the problem of induction. So far as the ultimate foundations of induction are concerned, the case is simply this, that we do not know how to validate any induction; that, rather, so far as our knowledge goes, every inductive inference rests ultimately on some article of faith.

Though no particular inductive argument has a firmer foundation than that of the inductive method itself, what I want to do is to examine the view that the inductive method may be applied to the problem of the external world in the same way it is applied in the empirical sciences to phenomena, and that, therefore, its results in this field warrant belief as well as do those in science.

Since the proposition that there is an external world is an hypothesis presented for verification, we must first give a statement of the requirements for an admissable hypothesis. Among them are consistency and simplicity which need not concern us,

for few, I believe, will maintain that an hypothesis of an external world need contain inconsistent propositions; and, as for simplicity, it functions, if at all, not as an absolute criterion but rather as relative in deciding between rival hypotheses which are probable.[3] There is, however, one criterion which is of first importance but seems to be almost universally misunderstood, when not neglected. It states that an hypothesis must have an antecedent finite probability, which means that it must be somewhat probable independent of or logically prior to the facts the hypothesis is invented to explain. This demand for a finite antecedent probability is deducible from the Multiplicative Axiom of Probability, which is, I suppose, as certain as anything we know about induction.[4]

But how is it satisfied? In most particular cases it may be possible to get the antecedent probability analogically from some previous law or generalization or hypothesis which is somewhat probable, but its original source is, so far as I know, the bare assumption that nature fulfills what usually parades in the books under the name of the Principle of the Uniformity of Nature, or the Principle of Induction. As to the best formulation of such

[3] Cf., E. J. Nelson, "A Note on Parsimony," *Philosophy of Science*, Vol. 3, No. 1, Jan., 1936, pp. 62 ff.

[4] This deduction is as follows: The Multiplicative Axiom is: $pq/h = p/h \cdot q/ph = q/h \cdot p/qh$. Replacing p by C which is a consequence of hypothesis H, and q by H, we get: $C/h \cdot H/Ch = H/h \cdot C/Hh$. Hence,

$$H/Ch = \frac{H/h \cdot C/Hh}{C/h}.$$

H/h represents the antecedent probability in question. Only if the probability of H independent of C be finite will H/Ch have a value or will it be possible for H to increase in probability by a verification of its consequence C. (Though C/Hh is greater than C/h, since H is favorable to C, the common assertion that H implies C or that $C/Hh = 1$ is seldom if ever true in actual cases of induction unless H is a generalization. *E.g.*, the proposition that the victim was shot by a revolver known to have belonged to the accused is not implied by the hypothesis that the accused committed the crime. It will not help to extend H so that C/Hh will more nearly equal 1, for then H/h would be proportionately decreased.)

If acceptance of this axiom be questioned, I should reply that it is explicative of the notion of probability. Though we may not know the definition of probability, I think we do know this, that no proposed definition not implying it would be acceptable: it is a postulate of probability and so systematically defines in part that notion.

a principle logicians are not agreed. Fortunately it suffices for our purposes to say that it must imply that some events of some types are correlated with events of some other types, and that this correlation is not simply a matter of history, but is a reliable or stable correlation, grounded in some real connection holding directly or indirectly between the events.[5]

Against my assertion that this principle is a bare assumption, it might be argued that the expectation, *e.g.*, that tomorrow fire will cause heat, is justified by the mere fact that these two phenomena have stood in a certain functional relation in the past. A more general form of this argument is to infer from the successes of induction in the past its success in the future, but surely this itself is a case of induction, and so can hardly be a valid argument supporting induction. Such arguments beg the very question at issue.[6]

And then there is that possibility of chance, that haunts every inference from experience. Maybe empirical correlations are simply chance runs of events. Against this possibility I am acquainted with no sound argument. We must not forget that *a priori* the probability of any specified sequence is precisely that of any other having the same number of terms. Moreover, if events have occurred throughout an infinite past, it would be infinitely probable that any given sequence or repetition of sequences would sometime or other take place. *E.g.*, if God spun a roulette wheel for a period of time longer than any finite duration He can mention, He surely would not be surprised to get a finite run as long as He pleases. In general, as a chance series of events is continued the probability that any given finite sequence will occur sometime in the series approaches 1 or certainty as a limit.[7] Accordingly, the existence of runs or cor-

[5] Discussions of the precise formulation of a Principle of Induction so that the required antecedent probability will be finite may be found in Keynes, *A Treatise on Probability*, and in Nicod, *Le problème logique de l'induction*. (The English translation of this latter work is so inaccurate and misleading that philosophers should be warned to avoid it.)

[6] Apology for mentioning this hackneyed point would be in order were it not that some pragmatists seem not yet to have learned from Mill's blunder on this issue.

[7] Charles S. Peirce seemed to think that if a chance series be continued indefinitely any given order or sequence would certainly take place. He said, "It is an indubitable result

relations is quite as reasonable on an hypothesis of chance as on one of genuine law.

It must not be thought that the question is whether events in the future will occur in orders, *i.e.*, stand in functional relations. We can know that they will if we can know that there will be events, for it is impossible to find a finite series of events which theoretically cannot be shown to exemplify some function or law. The question is whether the fact that past events exemplify a given function is a *ground* for believing that future events will exemplify *that* function.

Hence, if we are to be able to learn from experience, or to predict with some assurance the future on the evidence of the past, there must be some reliable orders or connections between events. That there is implied by the Principle of Induction; and this is, I believe, the minimum assumption required in inductive inference.

Before applying these findings concerning induction to our problem, we must distinguish two types of hypothesis:

(1) The first type is of a law, or generalization. *E.g.*, from the correlation of many cases of *A* and *B* in the past, we conclude that all cases of *A* and *B* are so correlated. Or, in other words, from the fact that events of the type *A*, *B*, *C*, etc., have been observed to exemplify, within certain limits, a particular function, we infer that all events of these types stand to one another in this functional relationship.

In such cases the inference rests (a) upon the fact of past correlations of some cases of such events, and (b) upon the acceptance of a Principle of Induction.

(2) The second type of hypothesis is that of the existence of a thing or of the occurrence of an event. *E.g.*, the hypothesis

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of the theory of probabilities that every gambler, if he continues long enough, must ultimately be ruined." *Collected Papers*, vol. III, p. 396. Since Peirce surely does not mean to beg the question by the phrase "long enough," I cannot see that his assertion is quite correct. The most we can say, it seems to me, is that as a gambler continues the probability that he will ultimately be ruined approaches certainty as a limit but will never reach it.

of the existence of Neptune was invented in order to explain certain observed discrepancies in the orbit of Uranus.

In such a case the inference rests first upon some hypothesis of the first type—*i.e.*, upon some functional relation already inferred to hold between events or things satisfying certain descriptions, *e.g.*, in our illustration the law of gravitation. And it rests further upon knowledge of the existence of something which can supply values to one of the variables in the given function, *e.g.*, the perturbations in the orbit of Uranus.

5. *Application to our Problem.* From these examples and our explanations, I think it is clear that the hypothesis of an external world is of the second type. The phenomenal world or events therein function in this case as did the discrepancies in the orbit of Uranus: it consists of the events or things to be accounted for. The external world, like Neptune, is the thing hypothesized.

Now, are the above requirements satisfied by the hypothesis of an external world? In the first place, let us ask, what is and whence comes the law corresponding to the law of gravitation in our illustration? That is, have we a law or generalization *previously* established as probable by virtue of which the external world *would* explain the phenomenal world? There are two possible sources of such a law: (1) Empirical observation, and (2) The Principle of Induction.

(1) In order for such a law to be established empirically, we must have had experience of a relevant correlation between some phenomenal event and some event or thing in the external world, on the basis of which to generalize; which, however, is, in the very nature of the case, impossible. Even though we grant that we know some reliable correlations, still they are between parts of, or events in, the phenomenal world. We have no case of a correlation of an experiential event with an event in an external world, and in the nature of the problem we cannot have such a case. And more than this, just a functional relation would not suffice, because its stability would not be probable. A necessary connection of some sort is required. Though, of course, if it were probable that such a functional relation holds between ob-

jects of certain kinds, and if induction were known to be valid in the realm of such objects, it would then be probable that this functional relation is grounded in a necessary connection.

Or (2) it might be thought that the Principle of Induction would render it somewhat probable that there is such a law, for, it may be asked, does not this principle imply that it is probable that any randomly selected event is connected with any other event? No, for all that empirical science needs is that the principle provide that some *phenomena* be connected with some other *phenomena*. To extend this principle to include events in an external world would be to beg the question.

Moreover, even if this problem of a connection between phenomena and events in an external world were solved, still we should have to validate the hypothesis of the existence of such a world. We have found that an hypothesis may be a valid explanation of given facts, only if it be finitely probable logically prior to these facts. And since in our problem these facts are the phenomenal world or events therein, the hypothesis of an external world must be finitely probable on evidence or knowledge of facts not including our knowledge of the phenomenal world or phenomenal events. But how this is possible, since all evidence or known facts on which inductive investigations rest are themselves phenomenal, I fail to see. And since we have already seen that such a finite probability does not follow from the Principle of Induction, unless that principle be so extended as to beg the question, I can conclude nothing short of the proposition that the alleged inductive establishment of a probability of the existence of an external world is invalid, even granting that scientific inductions are valid.[8]

Another expression of this inductive argument, which sounds more plausible, is as follows: If there had been no external world grounding phenomenal events, then it is very improbable that cases of induction in the past would have been successful

[8] These two requirements, (1) of a finite probability for a law connecting the external and phenomenal worlds, and (2) of a finite probability for the existence of an external world, are the demands, respectively, that the value of C/Hh be finite and greater than C/h, and that H/h have a finite value. See footnote on p. 241.

(*e.g.*, prediction in physics and astronomy). But such inductions have worked remarkably well. Therefore, it is probable that there was an external world. Or, we may state the argument still otherwise, to-wit: Without some real ground the uniformities of experience would be highly improbable. Therefore the fact of uniformities renders it highly probable that such a ground exists. It might be profitable to consider this argument from two different points of view. (1) Let us consider the external world and its absence, or chance, as two rival hypotheses concerning the occurrence of phenomenal uniformities. Looking at it this way we find the argument to be a case of the use of the Inverse Principle of Probability, which De Morgan perhaps plausibly though erroneously expressed as follows: "Causes are likely or unlikely, just in the same proportion that it is likely or unlikely that observed events should follow from them. The most probable cause is that from which the observed event could most easily have arisen."[9] As Keynes has so clearly pointed out, De Morgan, along with Leplace and many other eminent authorities on probability, failed to see that, *only if* the antecedent probabilities of the alternative hypotheses be equal, do they drop out of the formula determining the relative probabilities of the hypotheses given the consequence. Moreover, the Inverse Principle requires that the antecedent probabilities be greater than 0, which requirement holds even though they be equal.

Now the proposed argument fails because, antecedent to phenomena, neither of the alternatives, (a) an external world, and (b) no external world, or chance, has any finite probability, for reasons I have already given in this paper.[10]

[9] "Essay on Probabilities," *Cabinet Encyclopoedia*, p. 27. Quoted from Keynes, *op. cit.*, p. 178. The Inverse Principle, which follows from the Multiplicative Axiom, is this:

$$\frac{a_1/bh}{a_2/bh} = \frac{b/a_1h}{b/a_2h} \cdot \frac{a_1/h}{a_2/h},$$

where a_1 and a_2 are possible hypotheses having the consequence b. Cf., Keynes, *op. cit.*, pp. 148f.

[10] Even if the Principle of Indifference be acceptable, appeal to it would not help here, for, as Keynes has shown, that principle is not applicable when the alternatives are of the form, p or $\bar{p}$.

(2) The second point of view is to omit chance as a rival explanation and to consider simply that, according to the theorem,[11]

$$H/Ch = \frac{H/h \cdot C/Hh}{C/h},$$

if C/h (*i.e.*, the probability of phenomenal uniformities independent of an external world) be small, then H/Ch (*i.e.*, the probability of an external world given phenomenal uniformities) is relatively large. The difficulty here is that this consequence follows only if H/h (*i.e.*, the probability of an external world independent of phenomenal uniformities) be greater than 0. Hence, for reasons given under (1) above, the argument from this point of view is invalid too.

To summarize the points of failure in the argument we have been examining: Even if induction as a method within the realm of experience or science had been validated, still the hypothesis of an external world would not have the required antecedent probability. In particular, (1) that something external to phenomena exists is not probable antecedently to our knowledge of the phenomenal facts to be explained, and (2) neither has a stable connection between the phenomenal and the external been presented or analyzed nor is the existence of such a connection antecedently probable.

I should place little confidence in this conclusion if I could not find some explanation why some philosophers have fallen into this error. From what I gather reading them, they seem to think that the method of hypothesis consists in this, that if a consequence of a supposition be true, or probably true, then that supposition is probably true, and that that is the end of the matter. They fail to see that such a conclusion follows only if the supposition have a finite probability antecedent to the verification of the consequence.

6. *Metaphysical Remarks.* In concluding I wish to make two remarks on the metaphysical side of the problem. The first is that any reason for a Principle of Induction is a reason against

[11] See footnote on p. 241.

scientific positivism, or science without metaphysics. Since the connections sufficient to justify such a principle are not given in immediate experience, that such a principle is true is a metaphysical doctrine. A principle of induction demands a real order amongst the events to which it refers. That is to say, there must be genuine, real connections between events. Functional relations descriptive of past events are not dependable unless they have a ground. And unless this ground be some kind of connection within the phenomenal realm, which no one has discovered, we may call it an external world.[12] Hence, if scientific inference is to be valid, some kind of external world is necessary; but I see no way to affirm the antecedent of this proposition. In view of this point we may restate our argument in one sentence; namely, since the validity of induction presupposes an external world, the external world cannot be established inductively.

Finally, I wish to indicate the dependence of the solution of the problem of induction upon the solution of the metaphysical problem of the one and the many. A thoroughgoing monism in which all separation or discreteness of objects is swallowed up in undifferentiated unity destroys distinctions necessary for discursive thought. But the opposite extreme of pluralism destroys intelligibility too, for it denies the effective connections necessary to inductive inference. It should be noticed moreover that the mere establishment of the existence of an external world would suffice to validate induction only if the connectedness and stability required for induction hold in that world and in the system composed of it and phenomena.

In conclusion let me say that I have not tried to prove the nonexistence of an external world. That would be impossible. I have endeavored merely to show that the inductive argument for such a world has not been validated. I want to state further that the philosophic interest in the problem is, at least so far as I am concerned, not that of contesting common beliefs in an

[12] If phenomenal events be grounded in an external world, then it is to be expected that no direct connections can be found between them, for phenomena would then be simply epiphenomena of the external world. In other words, if there be such an external world grounding phenomena, the results of Hume's analysis are to be expected.

external world, but rather that of discovering the presuppositions of the types of experience and of the beliefs we actually do have, and seeking their metaphysical justification. Hence it is clear that I am unable to follow the Logical Positivists in asserting that propositions in speculative metaphysics are meaningless; for, though I should agree that such propositions are not verifiable by or amenable to the methods of the empirical sciences, I do not see that it then follows that they are amenable to no method. I must admit however that, though it is quite legitimate to ask for the presuppositions, or for the necessary conditions of the facts of experience, it is not clear to me how a proposition truly reporting experience, could have as a necessary condition or presupposition some proposition transcending experience and so involving terms standing for non-experiential objects.

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Outlines of an Empirical Theory of Meaning

BY

A. CORNELIUS BENJAMIN

IN WHAT follows I shall consider symbols only in their function as conveyors of meanings. That symbols have emotive and volitional properties as well, that they have elaborate and complicated relations to the self which uses them, that they are themselves physical counters, i.e., noises, visual objects, etc.,—all of these facts I recognize but choose to neglect. When symbols are considered merely as instruments for the transfer of meanings, only one important assumption is involved, viz., symbols which are precisely defined are to be preferred to symbols which either are undefined or are defined inadequately. In other words, the task of a theory of meaning is that of showing how symbols take on a maximum degree of precision in significance.

If we examine any symbol from this limited point of view we see immediately that it has two very important relations, both of which seem to contribute to its meaning. One is the relation of the symbol to its referent, which may here be called its *extension*; the other is the relation of the symbol to other symbols in the system, which may here be called its *intension*. This use of "intension" is somewhat broader than that usually accorded to the term, since it includes not merely relations of implication but of compatibility and incompatibility, contradiction, non-implication, and so on, through the gamut of possible syntactical

relations. Then the meaning of a symbol is determined by its extension, by its intension, or by both. The alternatives excluded are that meanings are determined through divine revelation, intuitive insight, innate sources, and the like.

An important question immediately arises: Is every symbol to the same degree both intensional and extensional, or are there symbols in which extension predominates over intension, and symbols in which intension predominates over extension? If we examine symbolic schemes and their referents, the answer to this question seems clear. There are symbols which are predominantly extensional, and symbols which are predominantly intensional. Among the former are the symbols for the so-called psychological simples of experience—tastes, sounds, colors, odors and touch sensations, singly or in combination, with which we have immediate acquaintance. The actual listing of such simples varies, unfortunately, from individual to individual, but the fact that there are such simples and symbols for them establishes the existence of symbols whose meanings cannot be wholly an affair of intension. The elaborate theory which certain idealists have constructed to make the contrary seem plausible does not impress me as sound. But there are also symbols whose meanings seem to be determined predominantly by intension, since the referents which they designate (if they designate referents at all) are not such as to permit of direct inspection. Among these symbols are some which might be called "semi-extensional" symbols: "the other side of the moon," "the center of the earth," "the consciousness of my fellowman"—in which the referent is an empirical object not accessible to observation—and purely intensional symbols: "implies," "$\sqrt{-1}$,"—in which there seems to be no referent of any kind. In the case of the predominantly extensional symbols the intensional relations seem to be secondary and incidental, and in the case of the predominantly intensional symbols the extensional relations seem to be secondary and incidental.

We may adopt a simple terminology to designate the results thus far achieved in our analysis of symbols. Symbols whose meanings are determined essentially by extension may be called

descriptive symbols. Symbols whose meanings are determined essentially by intension may be called *suppositional* symbols, since their referents are not given clearly. Suppositional symbols need not be vague or obscure as to meaning, since they may have precise relations to other symbols from which they get their meanings. We have shown that our total symbolic system contains both descriptive and suppositional symbols.

Granting that the fundamental postulate for the use of symbols is that symbols which are precisely defined are always to be preferred to symbols which are vaguely defined, we find ourselves confronted with another problem: How do symbols acquire the meanings which they possess, and to what source should we go in order to clear up any vagueness in the meanings of symbols?

The solution to this problem seems to offer no difficulties so long as we limit ourselves to descriptive symbols. If the meaning of a descriptive symbol is vague, we should turn immediately to that which is presumed to be its referent. For the meaning of a descriptive symbol is a function of its referent. (The meaning of a descriptive symbol is a function also of the operational route followed in passing from the referent to the symbol, but this additional complication need not concern us here.)

But the problem is more difficult in connection with intensional or suppositional symbols. The referents of these symbols are not given clearly, and may not be given at all. Hence the denotative route is closed. But such symbols, it may be maintained, have precise intensional relations to other symbols in the system, and may take on meaning through this route. In fact this is exactly the method which is employed in the act of definition; a symbol is given precision by revealing its relation to another symbol, presumably better known, within the same symbolic scheme. This is also the method employed when we clarify a postulate system by deriving a group of theorems from it, when we clarify a proposition by showing the postulates from which it can be deduced, and even when we clarify a symbol by indicating other symbols with which it is incompatible or contradictory. If a symbol is a part of a symbolic scheme then every structural

relation of the symbol is a part of its meaning, and may consequently be employed for the purposes of clarification.

Suppose, for the moment, we grant the legitimacy of this method of intensional definition. Two difficulties arise—difficulties which may not prove to be insuperable, but which must be met in order to give the method any final justification. I. How are the other symbols, in terms of which the clarification is made, themselves to be clarified? II. How can we be sure that the definitional route by which the clarification is accomplished is expressive of the actual relationship of the symbols in question? Let us attempt to see where each of these difficulties leads.

I

The symbols in terms of which the clarification is made may themselves be descriptive symbols, and will then take on precision through their referents. It will follow that suppositional symbols are not different in kind from descriptive symbols, but merely in their "distance" from the empirically given, i.e., descriptive symbols take on meaning through direct operations upon the empirically given, while suppositional symbols take on meaning through operations on symbols which are themselves obtained through direct operations upon the empirically given. Suppositional symbols may then be defined in terms of indirect or complex operations upon the empirically given.

This position has some plausibility. In fact it seems that some such method must be precisely that which is employed in defining those types of symbols which I called semi-extensional—"the other side of the moon," "the center of the earth," "the consciousness of my fellowman." Moreover this seems to be the method which is employed in defining all hypothetical and theoretical entities of science—molecules, atoms, electrons; we cannot define the symbols for these entities by direct inspection of the entities in question, since the entities are not clearly given; consequently we define the symbols in terms of their relations to symbols for entities which are given clearly—heat, differing elements, radiation. Again this seems to be the method which is employed in defining all idealizations of experience in so far

as they function in science, e.g., perfect levers and triangles, ideal gases, frictionless motion; here the defining operation is one of serial extension beyond the limits of an empirically given order.

But can the method be employed for the purpose of defining those types of symbols which I called *purely* intensional—"implies," "$\sqrt{-1}$"? Such symbols seem to have no empirical reference whatsoever, direct or indirect; propositions which contain them seem to say nothing about the world. The propositions of logic and mathematics are pure tautologies, deriving their truth from their analytic character. They are true because they follow from arbitrarily asserted definitions, not because they reveal the structures of objects. They are, from the empirical point of view, strictly meaningless, since they obtain their meaning through operations performed upon symbols taken as undefined.

It seems clear that if there are such symbols as these, both meaning and truth with reference to them must be an affair internal to the system made up of the symbols in question. No symbol can acquire meaning, and no proposition can acquire truth by virtue of a relation of the symbolic element to something out of the system. Thus purely intensional symbols cannot be defined in terms of operations performed upon other symbols which are themselves extensional in character, i.e., the term "implication" cannot be defined analogously to the term "the other side of the moon." Intensional symbols must be defined in terms of other *intensional* symbols; for example, implication must be defined in terms of disjunction and negation, or in terms of conjunction and negation. The system of logical symbols is a closed system, and no intensional symbol can have logical relations to an extensional symbol. I shall return to a consideration of the consequences of this sharp dualism later; for the present we must return to the second of the difficulties which arises in connection with the definition of intensional symbols.

II

How can we be sure that the definitional route by which the clarification is accomplished is expressive of the actual rela-

tionship of the symbols in question? If A is defined in terms of a certain relation to B, we can clarify A only if we know what B is and what the relation of A to B is. We have shown the difficulties that arise when we attempt to clarify B; we must now ask how the structural relationship is clarified. Usually we do not ask that it be clarified; we assume both its existence and its intelligibility. We define A by showing with what it is equivalent, what it implies, what implies it, with what it is compatible, incompatible, contradictory, etc. But in the act of definition we assume both that we know what equivalence, implication, compatibility, etc., mean, and that the symbol in question does have precisely these relationships. Clearly the assumption may be called into question on both grounds. We should then be confronted with the task of defining these symbols for logical relationships, and of justifying the assertion that they are the proper connectives in the system under examination. This reduces precisely to the difficulty considered under the preceding section, viz., the problem of defining purely intensional symbols.

But from another point of view the problem is different. At least the attempted solution is different. The problem of the definition of logical symbols may be met by insisting that they are themselves extensional symbols, and can therefore be defined in terms of their referents. The symbol "implication," for example, may be defined by means of some instance of it, say, "*pq* implies *p*." This is an attempt to adopt a strictly empirical attitude even with reference to logical notions. Logical propositions are considered as making assertions about symbols, and hence as being verifiable in terms of these symbols. But this does not seem to me to be adequate. We may, of course, make an assertion about a symbol, but this assertion may be one of two kinds: it may describe the symbol as a physical happening, e.g., as a word, or it may describe the meaning of the symbol, e.g., that it is equivalent to X, or implies Y. Now an assertion of the former kind is verified in terms of the word, and that is the end of it. But an assertion of the latter kind, although it is verified when we have shown that the symbol in question is equivalent to X, or implies Y, immediately creates a new problem of empirical verification, for *the fact that the symbol has these*

structural relations is a fact about the meaning of the symbol, and this demands clarification and corroboration. Hence one cannot carry out an ultimate clarification of the meaning of any symbol by pointing to the structural relations of the symbol to other symbols, for these relations are themselves symbols and demand clarification. This means that the problem of the definition of intensional symbols, both those which are semi-extensional and those which are purely intensional, rests upon a theory of the way in which the purely intensional symbols, i.e., the symbols for logical structure, themselves are defined.

III

I wish to propose that logical propositions be considered not as purely intensional symbols but as semi-extensional symbols. But such a theory has plausibility only in case the prevailing point of view can be shown to give rise to difficulties. Hence I wish to devote a few words to an examination of the consequences of the accepted viewpoint that logical propositions are tautologies.

If the propositions of logic are purely intensional symbols, the result is an irreconcilable dualism. Symbols become divisible into intensional and extensional; sciences into formal and material, *a priori* and *a posteriori*, non-existential and existential; propositions into analytic and synthetic; truth into consistency and correspondence; meaning and verifiability into formal and empirical. There can be no talk of reducing the formal to the material, or vice versa. The world exhibits this brute distinction, and that is all there is to it.

The plausibility in this position lies in the fact that there is a sense in which this ultimate dualism is undeniable. But it is not a dichotomy of kinds of symbols, or kinds of sciences, or kinds of propositions, or kinds of truth, or kinds of meaning and verifiability. It is a distinction between the two kinds of relation which every symbol possesses. Every symbol is both intensional and extensional, i.e., it has relations to other symbols within the symbolic scheme, and it has relations to the object which is considered its referent. This distinction is ultimate; neither

relation can be reduced to the other. But it is a distinction which applies to *every* symbol, and thus cannot be employed as a principle for dividing symbols into kinds. There are no symbols which are purely extensional and no symbols which are purely intensional. Proper names, which seem to be instances of the former, really have intensional relations; for example the word "this" is distinguishable from the word "that." And logical symbols, which seem to be instances of the latter, really have extensional relations, as was shown in the preceding section. Hence we are obliged to conclude that every symbol may, from one point of view, be considered as formal, *a priori*, non-existential, analytic, consistent—i.e., as intensional. But the same symbol may, from another point of view, be considered as material, *a posteriori*, existential, synthetic, corresponding—i.e., as extensional. And the distinction is ultimate in the sense that when we are considering a symbol from the one point of view, we are not at the same time considering it from the other point of view.

It seems possible in view of these considerations to show the truth and error in the assertion that the propositions of logic are tautologies. "The tautology of any law of logic is merely a special case of the general principle that what is true by definition cannot conceivably be false; it merely explicates, or follows from, a meaning which has been assigned, and requires nothing in particular about the universe or the facts of nature . . . there are no laws of logic, in the sense that there are laws of physics or biology; there are only certain analytic propositions, explicative of 'logical' meanings, and these serve as the 'principles' which thought or inference which involves these meanings must, in consistency, adhere to."[1] This is a clear-cut statement. But it suffers under a serious defect. It does not put the propositions of logic into a unique class. If a proposition becomes tautological when it is shown to follow from a definition, then any proposition whatsoever may be made tautological by simply making explicit the definition from which it can

[1] Lewis and Langford, *Symbolic Logic*, p. 211.

be deduced. And since definition is an arbitrary act we can in every case invent the required definition. As Stefansson points out[2] I can make the proposition "Ostriches bury their heads in the sand when pursued" true by the simple technique of defining ostriches as birds which do precisely this. I may then be no longer talking about the biological specimens found in the deserts, but that is quite irrelevant so far as *intensional* considerations are involved. And it is quite irrelevant to *any* symbol so long as I limit my concern to intensional considerations. Hence when one states that the propositions of logic are tautologies, he is correct so far as he is considering the symbols intensionally; but the question immediately arises as to whether propositions of logic, just as propositions about ostriches, may not be considered also extensionally.

A further element of plausibility is contributed to the view that logical propositions are tautologies by the fact that in the case of symbols which are predominantly intensional, the intensional relations are taken as regulative over the extensional. Symbols of this kind seem to be more significantly, more essentially, defined through the intensional route. In case the symbols are propositions their truths seem to be determined syntactically rather than through the facts which they claim to assert. Hence they can never be shown to be false, for they are true by arbitrarily assigned definition, and once this definition has been made explicit they case *by fiat* to have any extensional reference. This is the case not only with the laws of logic and propositions about ostriches but with all propositions whatsoever. But it is not due to anything inherent in the propositions; it is the result merely of our *decision* to let intension predominate over extension. In other cases, e.g., propositions about the "psychological simples," mentioned earlier in this paper, we decide to let extension predominate over intension; in the case of such a proposition as "red is like the sound of a trumpet," for example, we should consider any verification other than the empirical one highly artificial. But we do not forbid such verification, and it

[2] *The Standardization of Error*, p. 17.

is not too remote to hope that neurologists will some day construct a postulate scheme from which the proposition in question can be deduced. It will then become analytically true; but it will not cease to be empirically verifiable, even though it becomes an integral element of a deductive system. For every symbol is both intensional and extensional.

If the propositions of logic are purely intensional symbols, making no empirical assertion, then the applicability of logic to empirical facts becomes an inexplicable mystery. This is a problem which has disturbed all rationalists in the history of thought from Plato down to the logical positivists. The fact seems to be that we can and do use logical techniques to get from one "place" in nature to another. On the one hand we can start with propositions known to be empirically true, and by employing the proper deductive operations arrive at other propositions which prove to have been correct anticipations of nature. On the other hand we can start with propositions not known to be empirically true, and by employing the proper deductive operations arrive at other propositions which we know to be empirically true; these in turn transmit to the original propositions an increased probability which often proves to have been a correct anticipation of nature. In other words, we seem to be able by deductive and inductive techniques to expand progressively the range of empirically verifiable propositions. This seems to argue for the fact that our logic is a map which enables us to go successfully from one place in nature to another. How can this be explained if our map is not a map of nature?

The difficulty may be met by insisting that there is here no problem of explanation, for the fact to be explained is a pseudo-fact. Logic does not enable us to make discoveries; the propositions of logic are tautologies and we can never get novelty by means of them. Logic is not an instrument of discovery but of clarification. Whatever is deducible from a postulate set must have been contained implicitly in that set; deduction is an operation by which the implicit is made explicit.

Unfortunately this method of solving, or rather of avoiding, the problem cannot be refuted. In order to ascertain whether

deduction involves novelty we should be obliged to hold up the *implicans* and the *implicate* and compare them as to content. But we can never "get" the content of the *implicans*, for the content of the *implicans* is precisely the *implicate*. "If we wish to characterize the purport of a given sentence, its contents, its assertive power, so to speak, we have to regard the class of those sentences which are consequences of the given sentence.[3] We therefore refuse to say what a proposition means until we know all of its consequences, and it then becomes absurd to ask whether there is anything in the consequences that was not in the premises. If deduction is analytic then a proposition which is deducible cannot be novel, and a proposition which can be shown to be novel with reference to another proposition cannot have been deducible from it.[4]

Furthermore, if the content of a proposition is determined by its consequences then the content of the consequences must be determined by their consequences, and so on. If the consequences of a postulate set are infinite, then no definition of content is possible. If the consequences are not infinite, then the ultimate definition of content must be something other than consequences. This solution seems to be based upon the assumption that in any proposition, "*p* implies *q*," the kind of thing *q* is underived as to its content while the kind of thing *p* is derived. I see no reason for this assumption other than the arbitrary definition of implication upon which it is based.

But even if deduction is merely a technique of clarification, and not a map for getting about in nature, there is still the prob-

[3] R. Carnap, *Philosophy and Logical Syntax*, p. 56.

[4] The article by Leo Abraham, "A Note on the Fruitfulness of Deduction," *Philosophy of Science*, vol. 3, no. 2, does not seem to me to meet the issue squarely. Novelty in deduction may be due, according to this article, either to a "linguistic" feature, or to a neglect of one or more of the premises (or of their associative union) in making a comparison with the conclusion. But if deduction is itself a linguistic matter, how by means of such techniques can we *derive* a linguistic form which is regarded as "more significant" than the premises from which it was derived? And if deduction arises simply in the "joint consideration" of the premises, rather than in their single consideration, then the problem of novelty arises precisely in the act by which we pass from single to joint consideration. Propositions do not associate themselves, we associate them; thus by an act proceeding according to rule we get novelty.

lem of right and wrong techniques of clarification. Suppose I am attempting to verify the proposition "molecules exist." I proceed first to "clarify" the proposition by reducing it to its empirical consequences, viz. "bodies expand when heated; gases diffuse when placed together; etc., etc." Suppose I deduce a consequence which is not empirically verified. I immediately conclude that I have erroneously attributed a certain property to molecules, as a result of which the false proposition can be derived. Avoiding for the moment the problem of how I can have erroneously attributed a property to something which has no content until I have determined its consequences, there is a more significant difficulty. How is it possible to know that I have not *erroneously clarified* the hypothesis? If its consequences are not verified then either it is itself incorrectly defined, or the consequences which I suppose to follow from it do not follow. The former supposition is much more likely than the latter. But the latter is a significant possibility in case the consequence is a P-consequence in the Carnapian sense,[5] and it is only a slightly more remote possibility in case the consequence is an L-consequence. And just as the principles for the determination of P-consequences are empirical generalizations, corrected progressively by the accumulated evidence, so are also the principles for the determination of the L-consequences. If a scientific law is wrong it is corrected by the more specific data of nature; if a law of logic is wrong it is corrected by the more general data of nature—by the abstract interrelations of structure.

IV

It is at this point that I wish to state the claims of an empirical logic. It maintains that there are no purely intensional symbols, and that symbols which are predominantly intensional differ from symbols which are predominantly extensional only in the directness of the reference to the given; hence intensional symbols may be called semi-extensional. Every symbol is definable both extensionally and intensionally; but in the case of symbols

[5] Carnap, *op. cit.*, p. 52.

for clearly given objects extension is taken as regulative over intension, and in the case of all other symbols intension is taken as regulative over extension. However in the case of the latter type of symbols the regulation is only relative, for an intensional feature of a symbolic scheme is always definable ultimately in extensional terms. This empirical view of logic claims to dull the sharpness of the dualism between intension and extension, It insists that the division of sciences into formal and material, propositions into analytic and synthetic, concepts into *a priori* and *a posteriori*, criteria of meaning and verifiability into formal and material, are all relative to point of view. It insists that the formal is simply the most general material; that there may be analytic propositions in the social and biological sciences, and synthetic propositions in logic and mathematics; that every concept is *a priori* if considered as legislative of the given, but *a posteriori* if the given is considered as legislative of it; that the criteria of meaning and verifiability cannot be definitely formulated since the given is a continuum varying in clarity from a maximum to a minimum, and there is no point at which we can say conclusively that discourse has become nonsense.

The proper approach to an understanding of the empirical theory of meaning lies along something which may be called the natural history of suppositional symbols. In order to understand how suppositional symbols mean, it is well to consider the kind of situations out of which they arise, and the way in which they take on meanings through these situations.

Let us suppose that the symbol, "2 + 2 = 4," is a descriptive symbol in the sense mentioned above. It is then considered to be an empirical proposition with both its meaning and its truth determined by its referent. In other words, the symbol has been formed as descriptive of the fact that whenever we have two 2-groups we can combine them in a way called addition to produce a new group which we call a 4-group.

But we have never been satisfied, in the natural history of ideas, with mere *description*; we have sought *explanation* as well. We have felt that although it is important to know *that* 2 + 2 = 4 it is more important to know *why* this is so. So we set about to

construct a postulate system, such as that of Russell or Peano, from which we find it possible to deduce the proposition in question. It should be noted here that we consider the proposition which is derived extensionally to be identical with the proposition which is deducible from the postulates.

Now let us make one further assumption. Let us suppose something which even for the most extreme empiricist is quite unlikely, viz., that the proposition is refuted by a contradictory case. We shall suppose that we come upon a genuine case of $2 + 2 = 5$ or $2 + 2 = 3$. The descriptive proposition would no longer be true, since it is determined only extensionally. But it would still be deducible from its postulate system. We are thus led immediately to recognize that the proposition, "$2 + 2 = 4$," may not be a simple symbol but two symbols, one whose meaning and truth are determined extensionally, and another whose meaning and truth are determined intensionally.

The sharply dualistic attitude toward nature arises at precisely this point, and it is the result of a simple act of forgetting. We forget that to explain the empirical is not to explain it away. We forget that the postulate system in terms of which we explain the descriptive proposition is itself a suppositional symbol, and that we are therefore entitled to attribute to it only such meaning and such truth value as is justified by the character of the descriptive proposition and of the explanatory operation which called the postulate system into being. We forget that through this act the postulate system itself becomes empirical, differing not in kind from the descriptive proposition, but merely in the degree of removal from the empirically given. And we forget that any proposition deduced from this postulate set shares in this empirical character, and hence can be only *relatively* regulative over the descriptive proposition which was its ultimate origin and from which it soon becomes sharply distinguished. Finally, we forget in the generalization of this situation, that every symbol which appears to be only intensional in character must be definable ultimately in terms of extension, i.e., through a descriptive symbol and an operational route.

I feel that the greatest confusion has been introduced into the

history of thought by all of those who distinguish sharply between intensional and extensional symbols, and then proceed to engage in philosophical and scientific discourse without stating clearly in connection with every symbol which they use which type they are employing. If all philosophers had been complete rationalists or complete empiricists, there would have been no difficulty. For the rationalist a symbol has meaning only by virtue of its participation in a system, hence its extensional relations are non-existent or secondary; for the empiricist a symbol has meaning only because it designates things that are given, hence its intensional relations are non-existent or secondary. But for the empirico-rationalist symbols have relations both to objects and to other symbols, and, since these relations often vie with one another in the determination of meaning, we must know clearly in each case whether the intensional relations are to be taken as legislative over the extensional, or the reverse.

The empirical theory of meaning which I propose is based upon two fundamental propositions: (1) Symbols are to be defined through the extensional route, wherever this is possible. (2) Symbols which cannot be satisfactorily defined through the extensional route must be defined through the intensional route, viz., through their relations to symbols which can be defined through the extensional route. But this does not lead, as might be expected, to a division of symbols into two sharply differentiated groups, extensional symbols and intensional symbols. For intensional symbols are simply extensional symbols whose "distance" from the given is great rather than small. Since the limits of the denotable are not given with any degree of precision, we must recognize the impossibility of stating with certainty in any case whether a symbol is extensionally or only intensionally definable. Probably the simplest denotative act requires an operation of some sort, e.g., gesturing, or focusing of attention, from which such elaborate operational techniques as, say, Whitehead's principle of extensive abstraction, differ only in degree. Furthermore the continuity between descriptive and suppositional symbols suggests both that the former have intensional relations and that the latter have direct extensional

relations. It suggests also that in descriptive symbols the extensional relations will predominate over the intensional, and that in suppositional symbols the intensional relations will predominate over the extensional. But if one accepts an empirical theory of meaning, the former will be an absolute predominance, and the latter will be a relative predominance. If things are given clearly, symbols take on meanings directly from the things, regardless of the symbolic systems of which the symbols are parts. But if things are not given clearly, symbols for these things cannot take on meanings directly from them; hence we employ substitutes. We define suppositional symbols through the operational expansion of descriptive symbols, along lines which are expressive of pervasive structural features of the given. Intension predominates over extension only in the sense in which a knowledge of a portion of the given becomes a means for anticipating the character of another portion which is not so clearly given.

An empirical theory of meaning will be required to arrange all symbols into a series on the basis of their "operational distance" from certain clearly given entities which may be called empirical data. These operational routes will show both how the "remote" (so-called purely intensional) symbols are definable in terms of the data and how such symbols are explanatory of the data. Specifically, an empirical theory of meaning must show both how the laws of logic are definable in terms of clearly given empirical structures, and how the laws then become explanatory of these structures. It must show that the relation between the laws of logic and a certain empirically given structure is precisely the same as that between molecules and certain empirically given data such as heat, diffusion, etc. But it must then go on to show how the logical laws, because of their highly general character which makes their refutation improbable, tend to be looked upon as *purely* syntactical; since they make assertions about all things, they tend to become principles for the *use* of symbols. They are the principles which we must obey if we are to play the game. Hence they become regulative and inviolable. And having taken on this fixity they themselves de-

mand explanation. Hence we search among them for some which may be taken as undefined, and in terms of these and certain basic operations we endeavor to give the whole structure a deductive form. This merely increases the inviolability of the scheme, for every element has not only lost its empirical connection but has acquired a new intensional relation in terms of which its truth and meaning can be ascertained. Hence it becomes analytic and tautological.

It has always seemed to me that a distinction in degree is fundamentally more intelligible than a distinction in kind. If two things differ in kind they are distinguishable only through the dichotomy positive-negative. But in such a case the negative term always suffers in clarity; we know what it is not, but not what it is. The dichotomies of intension-extension, formal-material, *a priori-a posteriori*, analytic-synthetic, and all of the others based upon these, have always struck me as laboring under this vagueness. On the other hand if things are distinguishable in degree, one can "see" the nature of the distinction somewhat more clearly. For there is a continuum of stages separating one from the other, and even when the two are looked upon as limits of the series and thus as being in a sense outside of the series, the occurrence of elements in the series differing from them by small degrees mitigates the irrationality. Though the elements may differ greatly from one another there is at least the serial principle in which they both participate equally. It would seem helpful if we could reduce the above dichotomies to differences in degree. I have attempted to raise certain considerations which would argue for such a continuity.

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Purpose, Design and Physical Relativity

BY

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IN A recent issue of *Philosophy of Science* Merrit H. Moore[1] contends that it is not only possible but methodologically desirable to separate design in nature from purpose. The main part of his argument is devoted to a support of the proposition that "design" is objective, by which he means that design in the physical world is independent of mind. That which gives interest to Mr. Moore's argument is essentially the Kantian doctrine that the forms of the understanding, and consequently of knowledge, are furnished by the mind and are, in a sense, imposed upon sense-experience thereby making them rational. In contrast to the Kantian view stands the Humean doctrine that knowledge is analyzable into and limited by sense-impressions.

Mr. Moore rebels against the doctrine that design is imposed upon the world, and he is also opposed to the Positivistic doctrine which Humeanism led to, namely that facts of nature are causally disconnected. Rather, he points out that it is impossible to have knowledge of the physical world either by treating it as if it were affected by the knower through the imposition of an order or a form upon it, or by treating it as if its parts were causally independent of each other, or by treating it as if its

[1] See his article "A Metaphysics of Design without Purpose." Philosophy of Science, Jan. 1936.

organization into wholes and parts were brought about by conscious ends.

I should like to agree with Mr. Moore that purpose is limited to the reflective thinker, but in view of present-day scientific theory the separation of purpose and design is not nearly so easily accomplished as Mr. Moore would lead us to believe. The theory of relativity has re-opened the discussion of the possibility of such a separation. Whitehead, Einstein, and Eddington have well brought out the difficulties in attempting to find an objective order in nature free from mind or from teleology. True enough, Whitehead's earlier works[2] are designed to explain how it is possible to have a knowledge of "nature closed to mind." By "closed to mind" Whitehead meant "unaffected by the observer." According to these earlier works the external world had little if any teleology within it, but, as brought out in *Science and the Modern World* and *Process and Reality*, an essential weakness of Whitehead's earlier treatments is that they omitted teleology as an essential factor in nature. Consequently he develops the theory that the prehending subject previses or envisages possible forms of actualization, and on the basis of the intensification and compatibility of these forms the subject selects some and rejects other, whereupon those selected are prehended into unity or are actualized. I shall merely point out what the reader already knows about the views of Einstein and Eddington. On the basis of the inability to detect any motion of the earth with respect to the ether, Einstein has generalized the Newtonian contention that the measured velocity of bodies is relative. This metaphysical generalization denies that there is an absolute difference between rest and motion and it takes all possible meaning out of "changes as such." Furthermore, inasmuch as there is "motion" and "rest" only with respect to definite objects of reference and not with reference to an absolute space, Einstein insists on the necessity of choosing such a body of reference before rest and motion can assure any

[2] *The Concept of Nature*, *The Principles of Natural Knowledge*, and *The Principle of Relativity*. Hereafter these works shall be referred to as *Concept*, *Enquiry*, and *Relativity*, respectively.

actual values whatever. Such a choice, according to Einstein, implies that space-time systems may be imposed upon the Minkowki world of events, by the mind, thereby giving the events definite spatio-temporal orders which makes it possible for us to give definite numerical values to the velocities of the bodies under consideration. But these numerical values are relative to and limited by the perspective chosen by the observer. The naïve reply of the objectivist that, "after all, once the choice of a reference system has been made, then the motions of the bodies in question are quite independent of the observer, and really such motion is only discovered and not created by the observer" is altogether inadequate as an answer to Einstein's position. The question is, What is the meaning of qualitative change in nature as such independent of a *chosen* reference system? This question is intimately related to Mr. Moore's problem, namely, What is the meaning of design in separation from purpose?

Let us clarify the as yet obscure relationship between these two problems by an explanation of the relationship between the theory of physical relativity and teleology in nature.

As indicated above, Einstein and Eddington hold the view that order in the external world is dependent upon a mind, for as such the Minkowski four-dimensional world of events may be spoken of as having spatio-temporal intervals between and among the events in it, but these intervals merely represent the potentiality of events for alternative space-time systems which are applicable to them. Such an application however, requires an ordering of the passage of events but these orders are not in world of events as such. Rather, according to these thinkers, order is furnished by the mind.[3]

Let us give an example of such an ordering of events. If we assume that motion and rest are relative terms at the best, then it is quite as logical to conceive of the sun, moon, and

[3] Eddington, A. S.: *The Nature of the Physical World*, pp. 239, 264, 278. Einstein, A. And Others: "The Foundation of the General Theory of Relativity," in *The Principle of Relativity*, pp. 111f.

stars as revolving about the earth as a center as it is to conceive of the sun as the center of our planetary system with the planets revolving about it. In this case, of course we are not assuming that the Newtonian laws of gravitation hold necessarily. Furthermore the principle of simplicity properly belongs to methodology and not to metaphysics. Although this principle may be identified with the principle called "Occam's Razor," it should never be employed at the expense of logical consistency. The Ptolemaic worldview was not discarded merely because of complications of explanation which it lead to; rather experimental evidence would not permit an approval of it.

Now we are ready to ask, Which of these two incompatible designs, the Ptolemaic geocentric universe or the Copernican heleocentric planetary system, is objective? Since Mr. Moore means by "objective" that which is independent of teleology and of mind, we may ask, Which of these incompatible designs is independent of mind? Apparently it is exceedingly difficult for contemporary philosophers to isolate the metaphysical and epistemological problems introduced by the physical theory of relativity. The following simple illustration may be helpful in pointing out two of these problems.

Suppose that we grant that all motion is relative to physical bodies and not to Newtonian absolute space. If that is granted, then from an objective impersonal point of view and from the point of view of a mathematical statement of what happens, there is no difference in meaning between the statements, "the man moves toward the wall" and "the wall moves toward the man." The resulting metaphysical problem is that of giving meaning to change or happenings as such, for in the illustration given it is consistent to think of the man as having moved or as having remained at rest. Likewise, we may consistently consider the wall as having moved or as having remained in a state of rest. Which object, then, "really" moved? Or is motion *not* an ultimate fact of nature! If motion *is* an ultimate fact of nature, we must be ready to defend the proposition that motion is absolute. If motion is not an ultimate fact of nature, then a defender of this proposition must be prepared to explain the conditions under which there is motion at all in nature.

The acknowledgment of an unordered Minkowski world of events does not and logically cannot bring with it the implication that motion is an objective ultimate fact of nature. By this I mean that the application of the Lorentz transformation formulae to problems in the physical world is in a sense a flat denial of motion as an ultimate fact. These formulae deal with spatio-temporal intervals which, as such, are free from absolute numerical values. The problem at hand is, Under what conditions can the celestial bodies be said to assume order or acquire design? Apparently the Kantian doctrine that such order must be furnished by the mind has been accepted in the main by Einstein and Eddington.

The metaphysical problem concerning the objective, ultimate reality of motion brings with it an epistemological problem. Namely, is our knowledge of motion, of processes, and consequently, of "laws of nature" a knowledge unaffected by the knower? It is indeed impossible, from the point of view of relativity, for one to *know* that the sun does not or that it does revolve about the earth as a center. We cannot hold both that motion is an ultimate fact of nature and that from different perspectives (or attitudes or reference-systems) the sun does and does not revolve about the earth. However we can conclude with certainty that (i) if motion is an ultimate fact (which has been denied by man relativists), then we may have knowledge of the laws of motion which are not conditioned by the mind; but motion in that case must be dealt with as an inherent quality of bodies as such irrespective of other bodies or of absolute space; (ii) if motion is not an ultimate fact of nature, then we cannot have knowledge of processes and laws of nature which are objective, *i.e.* which are in nature independently of the knower.

This brings us face to face with the problem of the possibility of knowing a design in nature which is objective in Mr. Moore's sense. I take it that Einstein's and Eddington's interpretation of the physical theory of relativity is that there is no such thing as motion as such in nature, and that, although relative to chosen reference systems, there is such a thing as motion, motion depends upon a mind which selects a reference-system. Certainly these reference-systems are not all in the physical world prior

to selection, they would maintain; for if these systems were in nature, then they would be independent of the bodies considered, which would mean that all of them, though incompatible, would be absolute. That is, in the case of the man and the wall, there would be a reference-system in which the man was at rest and one in which he was moving. If this were the case both these systems would be independent of the bodies as such, which is to say, both would be absolute in the Newtonian sense.

Although we have brought out the problem of knowing a design independently of mind, there yet remains the problem of showing whether or not there is a design in nature which is free from teleology or from the influence of conscious ends. Probably Whitehead is the most outstanding contemporary champion of the view that design in nature cannot be separated from teleology. He shall not be treated here merely because he holds this point of view, but because the problems around which his philosophy of organism is focused have arisen for the most part out of an attempt to reconcile his realistic position in philosophy with the theory of physical relativity. The position which Whitehead defended in *Concept*, *Enquiry* and *Relativity*, is strikingly similar to, if not altogether identical with, that of Mr. Moore; for in these writings he holds that "nature is closed to mind."[4] In his later works, however, Whitehead accepts the view that natural processes cannot possibly be explained by the doctrine of mechanism and that teleology is an essential factor in any concrescent process. He writes:

> Finally, to sum up this train of thought, the underlying activity as conceived apart from the fact of realization, has three types of envisagement. These are: first, the envisagement of eternal objects; secondly, the envisagement of possibilities of value in respect to the synthesis of eternal objects; and lastly, the envisagement of the actual matter of fact which must enter into the total situation which is achievable by the addition of the future.[5]

[4] *Concept*, p. 4.
[5] *Science and the Modern World*, p. 154.

In *Process and Reality* he writes:

> Each actuality is essentially bipolar, physical and mental, and the physical inheritance is essentially accompanied by a conceptual reaction partly conformed to, and partly introductory of, a relevant novel contrast, but always introducing emphasis, valuation, and purpose.[6]

Again:

> This is the conception of God, according to which he is considered as the outcome of creativity, as the foundation of order, and as the goal towards novelty. Order and novelty are but the instruments of his subjective aim. . . .[7]

It is quite evident to one who has studied either *Science and the Modern World* or *Process and Reality*, that Whitehead emphasizes envisagement and teleology as absolutely essential facts in any natural process whatsoever. His reasons for such a claim are not altogether self-evident, and at present, in order to explain why scientific findings may lead to the doctrine that there is purpose in the physical world, I should like to make clear what rôle the theory of relativity assumes in Whitehead's present view.

Let us go back, then, and consider the inadequacies of Whitehead's earlier position that "nature is closed to mind." It is interesting enough to find that Whitehead held also that "motion is a physical fact,"[8] which, from the point of view of relativity, is a corollary to the statement that "nature is closed to mind." For, as pointed out above, if design is not imposed upon nature by a mind, then there must be a sense in which motion is absolute. Whitehead has expressed this point admirably. "Motion presupposes rest. . . . Now you cannot have a theory of rest without in some sense admitting a theory of absolute position."[9] Now one of Whitehead's main self-appointed tasks in his earlier works is that of supporting the thesis that there are alternative space-time systems in nature and that they are absolutely inde-

[6] *Process and Reality*, p. 165.
[7] *Process and Reality*, p. 135.
[8] *Concept*, pp. 105ff.
[9] *Concept*, p. 105.

pendent of mind, but can be abstracted from events by the mind. Let us point out the striking similarity between this view and the view held by Mr. Moore, namely that there is design in nature independently of the knower. With respect to the meaning of order, pattern, structure, or design, Mr. Moore writes:

> Stating it more broadly, it is the notion of some or all aspects of reality in such a way that the discovery and analysis of one part, leads to the discovery and knowledge of other parts.[10]

Before criticizing this concept of design, let us compare it briefly with that of Whitehead. He writes:

> Position in space is merely the expression of diversity of relations to alternative time-systems. Order in space is merely the reflection into the space of one time-system of the time-orders of alternative time-systems.[11]

The criticism to be offered to this point of view is: first, it does not account for the uniqueness of any given space-time system or, in terms of organism, it does not account for the uniqueness of the parts of a whole; secondly, it does not account for process or for the progressive integration of nature. That is, such a view precludes the ultimate reality of process and also of induction, for it implies that significance[12] and knowledge are a result of analysis of the parts which reflect the design of the whole; lastly, it explains each specific space-time system as actually composed of alternative space-time systems. This criticism seems to have been acknowledged as valid in Whitehead's later works. We shall discuss these inadequacies more in detail in order to make the difficulties of this view more explicit.

1. A definition of a part of the whole in terms of other parts of the whole. Suppose we argue in accordance with the doctrine

[10] "Design without Purpose," from *Philosophy of Science*, Jan., 1936, p. 4.

[11] *Relativity*, p. 8.

[12] Whitehead held that " 'significance' is an essential element in concrete experience." (*Enquiry*, p. 11). By this he meant that alternative space-time systems are signified by any given system because of their organic, constituent relationship to each other. He writes " 'significance' is the relatedness of things," etc. (p. 12).

of organism, that a particular space-time system is *merely* the reflection of other space-time systems. This would lead to the conclusion that alternative systems enter into any given system as an integral part of it. Now if there is any meaning whatever of the non-transitivity of simultaneity it is that alternative space-time systems are each unique in an essential respect. If this is the case it would be impossible for alternative time-systems to fully constitute a given system. There is, in other words, a uniqueness about every reference-system which cannot be attributed to a relationship between or among other such systems. The problem which immediately presents itself is that of getting out of a given order into another, or of inferring the nature of one order from the nature of the internal constitution of another. Again, it is the problem of inferring the nature of the whole from the nature of the parts which are in an essential respect unique. In *Science and the Modern World*, we find a definite acknowledgment of this difficulty, and the concept of design is expressed in the notion of eternal objects. Each eternal object is a "form of definiteness" or a pattern which may be grasped into unity, whereupon it becomes actualized by an event.

> Such an eternal object will be an ingredient of one event under the guise, or aspect, of qualifying another event. There is a reciprocity of aspects, and there are patterns of aspects. Each event corresponds to two such patterns; namely the pattern of aspects of other events which it grasps into its own unity, and the pattern of its aspects which other events severally grasp into their unities.[13]

What should be emphasized here is that (i) Whitehead acknowledges the uniqueness of every event which constitutes its intrinsic value and which is in that sense incommensurable. This precludes the plausibility of discovering the whole through an analysis of the parts unless we introduce some form of design which is in *all* the parts in a representative manner and possibly ideationally. (ii) In a sense eternal objects are possibilities for actualization before they have been prehended into unity. As

[13] *Science and the Modern World*, p. 151.

possibilities they are neither material nor physical, but are analogous to Platonic essenses which are envisaged; which is to say, they are the universals in Whitehead's doctrine of organism. They are repeatable and are, therefore, neither spatial nor temporal, but strikingly ideational in character.[14] The repetition of pattern throughout the parts of a whole is actually a repetition of designs which are present, not in actualized form, but only potentially and ideationally. Certainly there are no such things as patterns which have spatial characteristics. Patterns are "forms of definiteness" which are eternal; *i.e.* impervious to space and time. This interpretation is further substantiated in *Process and Reality*. There we find:

> That every prehension consists of three factors: (a) the 'subject' which is prehending, namely, the actual entity in which that prehension is a concrete element; (b) the datum which is prehended; (c) the 'subjective form' which is *how* that subject prehends that datum (p. 35).

Again: "A 'relation' between occasions is an eternal object illustrated in the complex of mutual prehensions by virtue of which those occasions constitute a nexus (p. 295). Hence it is conclusive that in Whitehead's system design cannot be separated from a prehending subject, and the representation of the whole in the parts is possible only through eternal objects which are neither temporal nor spatial. The prehension of envisaged eternal objects, of course, implies some sort of teleological factor.

2. The view that parts are constituted merely by other parts does not account for process and progressive integration.

It was explained above that Einstein and Eddington hold the view that space-time systems (the spatio-temporal orders of events) are possible only if there is a mind which, in a sense,

[14] In Whitehead's system it would be inconsistent to conceive of eternal objects as either spatial or temporal in character. Events are both spatial and temporal, and are that from which alternative space-time systems are abstracted. But since each spatio-temporal situation passes with the passage of its associated events (*i.e.* since there is no absolute space but rather each space passes never to recur) it would be logically impossible to attribute either spatial or temporal characters to eternal objects. Every eternal object has the capacity of "happening again." Since no moment and no space can happen again, these objects are free from such characters.

furnishes these systems and superimposes them upon events which have the capacity of submitting to alternative orders; *i.e.* the non-transitivity of simultaneity insures the applicability of alternative space-time systems. Throughout Whitehead's writings, however, he defends the position that the human mind in no manner affects the object of knowledge. This position, of course, does not carry with it the view that there is not teleology or purpose in the external world. Rather, as over against the view that space-time systems are superimposed upon events, he holds the view that space-time systems are abstracted from events. That is, the same set of events is capable of sustaining alternative space-time systems. This implies that motion is an ultimate fact of nature, and permits the possibility of processes in nature. Furthermore, such a view is consistent with the belief that there is progress in the world independent of the human mind. Let us explain why Whitehead is unable to account for motion and process without some sort of purpose or teleology.

According to the view that space-time systems are imposed upon events by a mind which chooses to consider the events from a particular reference-system, it is easy to understand what is meant by a geocentric universe. Furthermore, one can readily grasp the meaning of a heleocentric planetary system. But inasmuch as the results of the Michelson-Morley experiment are a flat denial of the reality of Newtonian absolute space, it is indeed difficult to conceive of an order of the passage of events as such independently of a Ptolemy or a Copernicus. But if one insists that there is such a thing as motion ultimately (*i.e.* independently of an observer and a mind), then one must also defend the proposition that there are timeless spaces in nature quite analogous to the timeless space of the Newtonian world; *i.e.* absolute space. Whitehead *does* defend this proposition. He writes: "Motion is essentially a relation between some object of nature and the one timeless space of a time-system"[15] and again:

> Our problem is to determine those relations between events which issue in the property of absolute position in a timeless space. This is in fact

[15] *The Concept of Nature*, p. 177.

the problem of the determination of the very meaning of the timeless spaces of physical science.[16]

At this juncture the inadequacies of Whitehead's earlier view should be pointed out. In *Concept*, *Enquiry*, and *Relativity*, Whitehead attempted to establish an empirical basis for the concept of simultaneity. Inasmuch as sense-awareness, according to the doctrine which he then held, discloses natural, objective relations independent of the mind and which emerge out of a relationship between events of which the human organism is a member (*i.e.* the percipient event), Whitehead held that simultaneity was a natural relation open to sense-awareness. Consider, for example, the specious present of a percipient event. Within that specious present we see, hear, taste, feel, and etc. All that is so sensed during a given specious present is said to be simultaneous with the percipient event during that specious present. Furthermore, a given event and all events simultaneous with it constitute a concrete slab of nature which is unlimited spatially and limited temporally by the durational life of the given event. Also, the given event which so limits the duration of a concrete slab of nature is said to be "cogredient" with that slab of nature. That is, congredience signifies rest, and if an event is cogredient with a concrete slab of nature, then it prescribes the "here" and the "now"; *i.e.* it defines the reference system with respect to which other objects are at rest or in motion.[17] During a specious present wherein the percipient event is aware of sense-data it is cogredient with a set of events. That is, in sense-awareness the percipient event is at rest and defines that reference system which gives objective reality to motion of which we are sensibly aware. In this manner Whitehead establishes an empirical basis for motion as an ultimate fact of nature. The reader must bear in mind that the percipient event is a natural event of the same ontological status as the life-history of, say, a pyramid, and that sense-awareness is free from all subjective, mental entanglements.

[16] *The Concept of Nature*, p. 106. See also Broad, C. D.: "*Scientific Thought*," pp. 96ff.
[17] See *Concept*, pp. 109, 110, 188; *The Principle of Relativity*, p. 25.

But what is wrong with this method of attempting to establish a timeless space prerequisite to motion?[18] Briefly it is this: (i) To say that an event is cogredient with a duration means that all of the events within that duration are "here-now" with respect to the cogredient event. But if we assume that there are spatio-temporal intervals between events, *i.e.* that they are separated in the space-time manifold, What is that which effects a togetherness of these events from the point of view of the cogredient event? Certainly the spatio-temporal standpoint as such cannot effect that togetherness (stratification of nature), for the general theory of perspectives or of stratifications of nature rests upon the assumption that the spatio-temporal factors in the world of events are commensurable. If, then, spaces are homogeneous in character, one cannot account for differences on the basis of differences in spatial location. There must be some sort of selection and rejection, inclusion and exclusion, of the world of events in order to effect a togetherness of them which is different from any other. Here "togetherness" may mean "perspective" or "stratification" of nature, and these perspectives are in a sense unique, and in a sense commensurable. That is, it is assumed by Whitehead that alternative space-time systems intersect, and in the intersection of two durations of different space-time systems we have a natural production of a level (plane), upon the intersect of two levels we have the production of a rect (line), and upon the intersection of a level and

[18] If an event is cogredient with a set of events then that event is said to be at rest absolutely with respect to the duration limiting its temporal extension. This means literally that the space of that duration (concrete slab of nature) is timeless, *i.e.* that space does not pass, but lasts through that duration. The moments, of course, pass away even as they do in the Newtonian system. Through this interpretation of rest and cogredience Whitehead is able to establish an objective basis for the separation of space from time, which, in the general relation of extension found among events, are together and interdependent as expressed by the term "space-time." Having once established a timeless space (of which there are many), which is analogous to Newtonian absolute space on a minature scale, Whitehead proceeds to define motion. One can see readily that motion for Newton meant the effective occupation by a body of contiguous points of absolute space at contiguous moments of absolute time. But if all spaces are relative; *i.e.* if they all pass with the passage of events, How can motion be an ultimate fact of nature? I.e. How can we know designs (laws of motion) in nature which are there independently of mind?

a rect, or two rects, we have a punct, which is the most elemental description of position and which, because of its production through intersections, insures the commensurability of alternative space-time systems. This, however, does not account for a particular stratification of nature (a particular duration). We must remember that a stratification of nature implies a certain design relative to a cogredient event. The problem is to account for that design without bringing in a mind or a subject or teleology of any sort. But Whitehead was unable to account for stratifications without introducing an act of prehension as an integral part of his system.[19] There is no use looking to the unordered world of events as such for an absolute order which is necessary if we hold that motion is an ultimate fact of nature. If we do not hold this view, however, we must hold that order is superimposed upon events. A prehending subject is that which effects the togetherness of events, thereby giving them a definite spatio-temporal order. Such a subject is not necessarily in mind, according to Whitehead, but is to be found in every event. Thus in *Science and the Modern World* Whitehead comes to the conclusion that design, order, and pattern, cannot be separated from a subject. In *Process and Reality* he concludes that design cannot be separated from teleology. (ii) This view does not and cannot account for the generic differences between internal and external relations. Unless there is a subject, there will be no qualitative differences between one stratification of nature and another, for spatial and temporal intervals are homogeneous and commensurable. But even if we grant the reality of a subject in prehension, there must be some sort of selection of eternal objects and other events lest a given subject prehend them all on an equal basis. If, however, every subject felt the same toward all other actual occasions and toward possible, envisaged eternal objects, then there would be no differences in the world. This would preclude the possibility of process, change, progress, as well as the emergence of novelty. Furthermore, if all prehensions were alike, there would be no basis for a

[19] *Science and the Modern World*, pp. 94, 101f., 213.

distinction between the relations called "external" and those called "internal." That is, all relations would be internal, and there would be no possibility of differences in the world. A mere difference in spatio-temporal standpoint is not a qualitative difference, for, as pointed out, such differences are commensurable. If we allow for uniqueness in the world, certain forms which are envisaged must be "prehended negatively"; that is, these forms may "add to the emotional complex, though not to the objective data."[20] Of course if there were no external relations then the internal constitution of each and every actual occasion would consist of the internal constitution of every other event. This would not permit of a subject in which these relations could be crystalized. (iii) If we do not allow for teleology in nature, there will be no reason for processes in nature or for change. Without teleology, what would be the stimulus for change? There is no use falling back on *mechanical force* to account for change, for in the first place, this will not account for the aptitudes of certain elements for others in chemical compounds. That is, this cannot account for "sensitivities" in the world, as Whitehead and Bacon well point out.[21] As a matter of fact, the mechanical interpretation of nature is just the point which has been put in question upon the innovation of the physical theory of relativity. Not only is purpose an essential constituent of every prehension, but there is a general purpose throughout all processes, namely that of intensification,[22] and progressive integration.

If motion is not an ultimate fact (not independent of mind) then certainly change is not an ultimate fact to those who believe that all change, both internal and external, can be accounted for on the basis of motion. But if change is not an ultimate fact, there is no such thing as external laws of nature. Yet if one holds that there may be no such thing as real change, but rather, that we have a parmenidean world, then it would be impossible to find design in that world since design would imply

[20] *Process and Reality*, p. 66. See also, pp. 35, 128, 162, 338–360.
[21] *Science and the Modern World*, pp. 60 ff.
[22] *Process and Reality*, pp. 75, 136, 155, 160, 248, 261.

differences. This, too, would preclude a discovery of the nature of some parts through an analysis of others. Our problem really becomes, therefore, one of accounting for change; but if the external world is free from all teleology, how can change bring with it anything other than chaos except by accident? Even in the laws of gravitational forces there is a mutual affinity for the bodies in a given system; But what is it that insures that constant presence of those affinities? That is, How can design be possible among a system of bodies indifferent to each other? If we grant that there is change and also design, how is it possible for the same design (pattern) to manifest itself at different times unless that pattern is neither temporal nor spatial, but similar to an eternal object? Whitehead has answered all of these questions by introducing purpose into the external world.

Mr. Moore implies[23] that teleology always brings with it some sort of "external compulsion." As Whitehead presents the matter, and I believe he is right, teleology never implies external compulsion. Rather "mechanical force" suggests such a compulsion, but teleology allows for freedom. As a matter of fact the actual occasions, which are the individual elements of the Whiteheadian cosmology as presented in *Process and Reality*, are composed each of feeling and a subjective aim.[24] This means that purpose is a constituent part of every actual occasion, but never an imposition from without. Furthermore, without purpose, integration, change, and design would be impossible.

3. Such a method attempts to explain specific space-time systems as composed merely of alternative space-time systems. That is, it is an attempt to discover the presences of other parts of a whole through analyzing a given part.[25] This problem is far-reaching. It involves among other factors, the nature of induction, the nature of deduction, and the meaning of the repetition of patterns in nature. At present I shall touch only on the meaning of the repetition of pattern, or design; but I suspect that Mr. Moore's conception of finding the wholes in the parts without admitting teleology, precludes the possibility of induction.

[23] *Ibid.*, p. 7.
[24] See *Process and Reality*, pp. 74, 75, 108, 134.
[25] See *Science and the Modern World*, pp. 174, 175, 183–5.

Most, if not all, physicists agree that the "world at an instant" is an abstraction. They will grant that existence involves time. *E.g.* it takes time for an atom to be an atom, for we must allow for a temporal span great enough for the electrons to whirl around the proton at least once. Also, a person cannot be himself in a time shorter than a specious present. This view is operative throughout all of Whitehead's writings. It was expressed less explicitly in his concept of "organism," but most admirably in his "epochal theory of time."[26] According to this theory time happens in "chunks," and differentiates itself from space through the endurance of pattern. There are two senses in which we may significantly interpret "repetition of pattern"; first, it may mean the existence of the same pattern at successive epochs of time; secondly, it may mean the existence of the same pattern throughout the various parts of a whole and at the same time. Let us consider these briefly.

(a) As explained above, a specious present of any event represents the minimum of time necessary for the existence of the event in question. Of course that present has past and future in it analytically, but there is no such thing as the existence of an atom of iron, say, in a duration shorter than its minimum specious present. But since any temporal spread is infinitely divisible, it is plausible to consider the specious present for an atom of iron as having "befores" and "afters," or phases within it. If the epochal theory of time implies that every phase of an epoch is contemporary with every other phase, or that there are no "befores" and "afters" within a given epoch, then there could be no such thing as passage, and although there may be the emergence of new epochs, we could have repetition of epochs at the best, but never passage or the endurance of patterns. The quantum theory, one phase of which gives assent to the possibility of an electron being in two places at the same time, would not permit a generalization of that peculiarity for which it is famous lest it preclude the possibility of different moments of time as measured by motion, which would result in a flat denial that "the same time" has any meaning whatever.

[26] *Science and the Modern World*, p. 183; *Proceedings of the Sixth International Congress of Philosophy*, 1926, p. 59f; *Process and Reality*, pp. 104, 434.

But during any phase of a specious present of an atom of iron there must be something which is cumulative and which finally brings the physical atom into actualization. Yet that which is present at any phase cannot be the physical atom itself, for it cannot exist in such a limited time as a fraction of its minimum specious present. Whitehead holds that it is the "pattern" which is so repeated at every phase.[27] But the pattern, an eternal object, is neither spatial nor temporal, and is prehended into unity by a subject. Now "prehension" is a species of "apprehension";[28] it is an uncognitive apprehension which involves envisagement, approval or rejection of form, and, of course, a purposive bringing together of eternal objects. By means of this teleological factor the repetition of pattern is not an acknowledgment of mechanism wherein repetition involves determinism and permits only of a repetition of the physical parts of the world. Rather the repetition of pattern accounts for endurance which is a background against which change takes place and gives meaning to novelty. (Of course, the mechanical doctrine rests upon the assumption that space and time are ultimately separable, and endurance signifies merely the passage of moments of time and the physical parts of the world are impervious to passage.)

(b) Repetition may mean the presence of the same pattern throughout the different parts of the whole at the same time. There is evidence that Whitehead's doctrine of organism implies this sort of repetition, especially as it is stated in his earlier writings.[29] It does not appear, however, as if this view is held to rigidly. Rather it seems to be denied in *Process and Reality* by the theory that "contemporary events happen in *causal* independence of each other" (p. 95). The theory of the causal independence of contemporary occasions lends support to the view that analysis of any part of a nexus of occasions would *not* reveal the nature of the contemporary whole. Such an analysis may lead to the discovery of the patterns of the past (the objectively immortal occasions) provided these patterns were prehended

[27] *Science and the Modern World*, pp. 175, 219.
[28] *Science and the Modern World*, p. 101.
[29] *Relativity*, pp. 8, 9, 24; *Science and the Modern World*, pp. 214–15.

positively by the present actual occasion. In any case the patterns or designs present in any actual occasion are neither spatial nor temporal, but are subject to reoccurrence in future actual occasions by virtue of an envisaging subject which may render the potential pattern into actuality.

In conclusion, the physical theory of relativity has made way for the introduction of two essential interpretations of the metaphysical character of design in nature. (1) That proposed by Einstein and Eddington essentially, which maintains that design is imposed upon the events of the Minkowski world. This view does not necessarily involve teleology, but holds firmly that design cannot be separated from mind. (2) The view held by Whitehead and his followers, namely that design cannot be separated from teleology, but that they are organically related. This view does not involve the mind, necessarily, but maintains that design is an objective fact; *i.e.* that motion, processes, and laws of nature are ultimately real.

Hence we are able to see that the rejection of Newtonian absolute space has given rise to significant metaphysical and epistemological difficulties. And although one may hold that design is objective, this does not imply that it is free from mind or purpose. If one attempts to apply the Kantian doctrine that space-time systems are furnished by the mind, this does not imply that the events of the world cannot support these systems. In case these systems are supported by events, they are still objective, but relative to the mind. If, on the other hand, one holds that design is objective and independent of mind, then a likely conclusion is that purpose must be introduced into the external world in the form of envisagement and uncognitive apprehension in order to account for a consistent repetition of patterns. Finally, the innovation of the physical theory of relativity has made it more difficult to separate either design and mind or design and teleology.

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The Logic of Science and Technology

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THE logician has not been conspicuous for his contributions to scientific knowledge, nor has the scientist been noted for his knowledge of logic. One might readily infer from these observations that there is no necessary connection between science and logic, but I think he would be in error. As one who makes no claim to being either a scientist or a logician, I am perhaps well qualified to say something significant about the logic of science!

I

There seems to be a current notion that every science has its own logic, but such a notion can hardly be maintained. One who had only read or heard the title of P. W. Bridgman's *The Logic of Modern Physics* might suppose that the book dealt with a special logic employed by modern physics; but he would read the book in vain if he were looking for such a logic. Rather, the book deals with the nature of modern physical concepts—as being operational in character—and with the apparently logical outcome of conceiving of physical concepts in this way. Hence, apparently, the title of the book. But physics is not unique in employing operational concepts, achieved by operational activities and thence serving as semantic substitutes for the things upon which the operations have been performed.

This is a characteristic of all the sciences. When physics, our best authenticated science, thus goes completely behavioristic, not only in conceiving of its fundamental concepts as having been developed through operational procedures, but also in conceiving that physical nature itself is processal, or constituted of variously patterned strains of behavior, we should perhaps be a bit slow about condemning the behaviorist movement in psychology. But, on the other hand, a behaviorism that incorporates the behaviorism of modern physics should not be identified with the atomistic, mechanistic behaviorism of John B. Watson. The logic of all our sciences, then, I assume to be essentially the same; and I assume also that the concepts upon which this logic is employed are operational in their getting and in their own essential nature. And I assume that this logic itself is behavioristic—a pattern of operational, space-time behavior.

II

The distinguishing mark claimed for scientific knowledge is that of universality, and experimentation constitutes the scientist's technique for making good on this claim. And "knowledge both new and true"—generally positive, but sometimes negative, and sometimes constituting an improved authentication of current knowledge—constitutes the mark of successful experimentation. But what the scientist will find in the course of his observations and experimentation will be largely determined by the interests, attitudes, or biases—his sense of wholes—with which he approaches his problems. These are largely a matter of historical and social conditioning, and they will even determine what sorts of problems he will isolate for solution, and the way in which he will envision them.

Thus Aristotle, whose mind had been formed by his living in an "upper stratum" of a class society, developed a logic of classes, or classification—a logic which well served the Church, with its hierarchical organization, which in turn, moulding the minds of the early modern scientists, resulted in the systematic and structural sciences, and the mechanistic philosophy of the last three centuries. Darwin and Lyell, walking together over

the same ground, did not see the same obvious facts. And Darwin, whose mind had been unwittingly formed at a safe remove from the ruthless competition incident to the Industrial Revolution and individualistic capitalism, saw throughout the organic world a similarly individualistic competition as a prime conditioning factor in organic evolution. On the other hand, Prince Kropotkin,[1] more independently alert to biological facts, and courageously able to rise above his social conditioning, saw that the struggle for existence was rather a competition among species, that this was most successfully carried on by a social coöperation within the respectively competing species, that the more social species were the more intelligent, and that this greater intelligence was the result of the greater sociality, and not the reverse.

Hence it is, through the social conditioning of its workers, that the universality of science in one age, even though scientific abilities, facilities and techniques might be equally good, appears as partiality or particularity in its successor. And hence it is that, with improved facilities and techniques, as well as broader interests, scientific knowledge must always be in process of revision—save possibly such as approves itself instrumentally, in technologies employing it.

I think the scientist does not fully appreciate the significance of his criterion of universality, even in the limited, Euclidean sense in which he has understood it. He tends to see only one of its two aspects. In the first place—the aspect that he does note, his knowledge of his particular kind or strain of facts must be true of "all" facts, of that kind. But the degree of his "truth," as we know today, is at best but a relative matter. His knowledge is only typically or statistically true. It is not absolute, as scores of generations, authoritarianly conditioned, have supposed—because of the authoritarians' insistence upon an absoluteness of their own "truth."

The second aspect of universality is equally relative, in part at least because of the social conditioning to which the scientist

[1] *Mutual Aid, A Factor of Evolution*, London, W. Heinemann, 1907.

has already been subjected—a matter just now referred to. In this respect, the scientist endeavors to serve as a reliable mouthpiece or spokesman of his kind of facts, to universal man. That is, he tries to describe or interpret his facts so truly that "all" men, with his superior equipment of training and apparatus, would be compelled by the nature of the facts themselves to report them as he does. He tries as hard as he can to say the last word upon them. He does not wish to suffer successful contradiction and consequent belittlement, at least in his own lifetime.

But even scientific universality must today take on an added burden of meaning, in both of the senses just described. Here we may take a cue from the gestalt psychology, with its insistence that perception is naturally of wholes. For the same sort of bias towards wholes appears in really scientific thinking, and it is from this bias that the interest in universality arises. We may actually say that a bias in favor of wholes, or of universality, is no bias at all, but actually constitutes the disinterestedness of science. Thus the primary and affirmative bias of a physician relative to his patients as individuals is constituted of a knowledge of whole or well persons, relative to their kind and degree of illness, respective ages, and consequent prospects of further life. He exhibits also a negative bias, relative to the illness of his patients, which latter constitutes a deviation from wholeness or health. In fact, a physician is in no adequate position for understanding either illness or treatment, save from the vantage ground of a knowledge of health or wholeness, in the temporally relative sense just indicated.

So, too, should it be with those who concern themselves with social illness. They should first be possessed of a bias in favor of universal humanity, as one single species or kindred strain of reality, and only secondarily in favor of constituent strains of the species—races, nations, institutions, and individuals. They should first know what a whole or well species would be—one that was unitedly devoted to the support, maintenance, and further continuance of its existence, and to such improvements in its habitat and its own inner constitution as would presumably

be instrumental to this furtherance of its life. This bias towards the spatio-temporal human whole is a prerequisite for any real social scientist or social technologist. It includes an active concern for all that will be born hereafter, as well as for those now alive. And it includes a knowledge concern for all that have lived, as affording both positive and negative cues for regarding the contemporary and the future. So, too, even the physician should be able to contemplate and deal with his patients in the light of a concern for the space-time human whole, of which these constitute but spatially and temporally localized functions.

Universality, then, is properly temporal as well as spatial in its range of inclusion. With our current knowledge background of evolutionary theory, informed by general and historical biology, comparative psychology, anthropology, and race psychology, we should be able to be more completely universal in our human biases, both spatially and temporally, than our predecessors could have been. Moreover, in the matter of all sorts of facts, we should become able to think of "existential" reality as being constituted of processes or strains, rather than of merely Euclidean classes; and of temporal segments of such strains as constituting functions rather than mere existences. Only so may the scientist justify his claim of serving as a mouthpiece of "all" his facts, of any given kind, to "all" men, even of his own day. Even universality, in the affirmative sense, can generally be but statistical in its range.

These two complementary aspects of universality, as thus revised and taken together, constitute the disinterestedness of science. The actual consummation or Major Term of the working scientist's endeavors will be his final success in making what he anticipates as a reliable and incontrovertible report of his facts, as an end product of his action; and his sentimentally or emotionally suffused concept of that product, as a current semantic substitute for that consummation, serves at once to motivate his act and to give it logical continuity throughout. This is a fact that the scientist himself does not generally recognize; and even less do those other unrecognized social technologists that we call theologians, legalists, and jurists.

III

For the scientist is actually a technologist, though he does not appreciate what sort of technology his is; and hence a logic of science is actually a logic of technology. His work constitutes a certain sort of intellectual participation in the life of the race so as to earn his right to live. Hence it is that it is not enough that he alone should know of his discoveries. "Knowledge for knowledge's sake" is sheer nonsense, for both society and the scientist himself. Knowledge that does not function, knowledge that makes no difference in the conduct of life, is no knowledge at all. Hence the scientist's propagating of his findings is a necessity of his occupation. There must be a public product or result of his procedures, both as a means for his own participation in the intellectual and consequently practical life of his kind and that society may know that he is earning his right to live. The notion of "pure science," "knowledge for its own sake," a knowledge that would make no difference in thought or conduct, was a defense gesture made necessary by an historical social control by the Church. The function of scientific knowledge is to serve as an instrumentality in a technology of living. Hence the "pure" scientist's work is not yet completed when he has merely concluded a successful diagnosis of some sort of situation—when he has merely substantiated an hypothesis regarding the nature of some set of facts. It is the conclusion of his work, a reporting of his findings to his fellow man, that makes his occupation a real technology; and if he does not render his report of his work, however good this may be, he cannot validate a claim to the title of "scientist."

IV

Most persons, when they think of logic, think of the Aristotelian and scholastic logic, and of the categorical type, exhibited in the historic syllogism about Socrates. The more logically wary or sophisticated, however, may also recall the hypothetical and the alternative syllogisms. The Aristotelian logic is supposed to rest upon three "Laws of Thought," called the Law of

Identity, the Law of Contradiction, and the Law of the Excluded Middle. The statement of these so-called laws was based upon the Platonic doctrine of Ideas, or the Aristotelian doctrine of Forms, both of which assumed that these Ideas or Forms, and their "objective embodiments," were absolutely fixed and distinct from each other. They assumed a static, Euclidean world. But modern physics, with its revelation of a processal world, gives the denial to all these "laws," relative to the objective references of our ideas or concepts; and modern psychology, revealing the relativity of concepts to current interests, likewise gives the denial to these laws as applying to the ideas themselves. Thus it is that Korzybski[2] and others are today interesting themselves in the development of a non-Aristotelian logic, appropriate for thinking about a non-Aristotelian, non-Euclidean, and non-Newtonian world. The three Laws of Thought are only statistically true, at best.

But it is not this aspect of the Aristotelian logic that interests me here; it is the fact that practical and technological problems simply cannot be solved by use of the Aristotelian logic alone. This is not a logic of forward-looking or intentional activity whether practical or technological. It is not a logic of exploration, discovery, or of the attainment of new knowledge. One cannot even put knowledge into Aristotelian, syllogistic form until the knowledge has already been achieved or learned. The Aristotelian logic, in its general social usage, is a logic of the backward look—a logic of concern for the maintenance of already accepted postulates, and for interests that are sheltered behind these postulates. It is here a saving, a conservative logic, and that is why it is so conspicuously used in theology, law, and jurisprudence—a misnomer is this word "jurisprudence," unless keeping one's eyes fixed upon historically accepted postulates, while he faces the future with his back, can be called "juristic foresight." But the theologian, legalist, and jurist are interested in security for the interests which they represent, and the only way they know by which this can be assured is by keeping things from changing—a vain effort.

[2] *Science and Sanity*, Lancaster, Pa., The Science Press Printing Company, 1933.

And now we can discern why it is that the canonical forms of the categorical syllogism, so highly favored by the theologian, legalist, and jurist, start off with the major premise—the one containing the Major Term. Instead of representing an effort towards gaining new knowledge, it is a case of applying old knowledge—which may be true or false—to new facts or situations. Thus Aristotle's famous syllogism was not an effort to prove that Socrates was a mortal; it was a case of showing how the concept of mortality could prove its efficacy, in application to Socrates. It is in a similar way, but with a further objective in view, that the theologian, legalist, and jurist employ their syllogisms. Thus they are actually employing also the forward-looking logic of technology, which I shall soon describe; but they are either unaware of the fact or successfully conceal it—perhaps even from themselves.

V

Now after this preliminary and rather negative spade work, let us proceed to the logic of achievement—the logic of exploration, discovery, creative effort; the logic of practical and technological endeavor. This logic has already been almost completely formulated, by Dewey, in his book, *How We Think*.[3] But Dewey was so intent upon science as a technique for arriving at new knowledge that he neglected consistently to follow the scientific process through to its actual consummation, and his logic is to that extent incomplete. He calls his logic a description of "the complete act of thought." If he had followed the thinker's act through to its consummation, he would probably have described his work as a description of "the complete thoughtful act." And in that case he might have furnished us with a complete logic of technology, which would incidentally cover the case of scientific experimentation.

Dewey failed also to provide his logic with a really logical terminology. It happens, however, that the terminology of the traditional categorical syllogism, with appropriate reinterpretation, precisely fits the completed Deweyan logic and thus serves

[3] D. C. Heath and Co., 1910; Revised edition, 1933.

to give it a better appearance as a real logic. The terminology here especially referred to consists of the technical expressions, Minor, Middle, and Major Term, and "extension" and "distribution." Moreover, as in the categorical syllogism, so here; each term appears twice in the solution of a problem—a thing which Dewey, and the theologian, legalist, and jurist have failed to observe. We can even employ here the traditional symbolism for terms—S, M, and P, as we shall see in a moment.

The English names for the three terms of a syllogism—Minor, Middle, and Major—all begin with M; hence the single initial letter of these names cannot be used to symbolize them all. It just happens, however, that the Conclusion always contains the Minor Term as grammatical Subject, and the Major Term as grammatical Predicate. Hence S, for Subject, and P, for Predicate, are used respectively to symbolize these two terms, both in the Conclusion and in the Premises. This leaves M free for use as a symbol for the Middle Term. Thus the categorical syllogism, in its four canonical "figures" relative to the pattern of succession of the Middle Term, is symbolized in the following formulae:

(1)	(2)	(3)	(4)
M——P	P——M	M——P	P——M
S——M	S——M	M——S	M——S
∴S——P	∴S——P	∴S——P	∴S——P

It is to be noted that in the course of actual syllogistic solution, represented by the two premises, the Minor and Major Terms appear once, while the Middle appears twice. Note also that the Fourth Figure is the only one that shows anything like a complete continuity of transition from one term to another, without any jumping about, even though—for the Aristotelian—this is the least preferred of all the figures, being sometimes described as a sort of *tour de force*, and not even recognized by Aristotle himself; but he employed a different principle for the determining of his Figures from that used today.

VI

As already indicated, the complete Deweyan logic is a space-time logic—a logic of movement or operation towards a preconceived end, terminus, or consummation. It would better be called a "psycho-logic," to borrow a term from Korzybski. It can be neatly illustrated by the use of a diagram,[4] and here the terms of a complete reflective act will be found to appear the same number of times, respectively, as they do in the premises of a categorical syllogism.

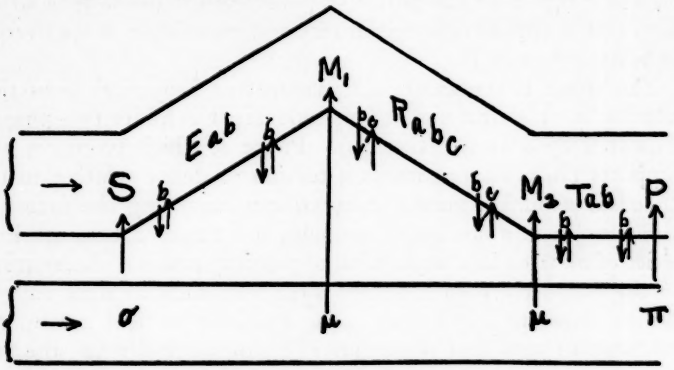

Diagram of a Complete Reflective Act

Let us now interpret the diagram. The space between the parallel straight lines represents the processal or temporal course of an "Event"—say an automobile or a human being—within which some deviation, σ, from wholeness of structure or of functioning has occurred. The space between the angular lines similarly represents in temporal or processal order the differentiated activities of an Agent involved in the solution of the problem generated for him by the deviation of the Event from wholeness. The horizontal space at the left represents a condition of habitude or complacency on the part of the Agent, prior

[4] For the original and basic concept of this diagram I am indebted to Professor R. D. Williams, of Ohio State University. Cp. his presentation of the diagram, in Williams, R. D. and Bellows, R. M., *Background of Contemporary Psychology*, p. 181; Harold L. Hedrick, Columbus, Ohio.

to the occurrence or noticing of σ, the break or lapse from wholeness in the Event. The occasion of the difficulty for the Agent, which is symbolized by σ, is reflected in terms of the Agent's perceptual experience as S; and so his problem is initiated. But it will not constitute a really intelligent and rational problem, unless the Agent can at this point conceive or envision, with some degree of definiteness, the kind of consummation or product of his activity, with which he would like to supplant his present difficulty. The symbols σ, μ, (the precise problematic detail of σ), and π represent the objective phases of the developing situation, which appear reflected in terms of experience, respectively, as S, M_1, M_2, and P.

The space E represents a movement of Empirical Induction relative to this situation. This movement exhibits two phases, indicated by *a* and *b* (Ea, Eb). Phase *a*, which promotes and supports phase *b*, constitutes a tension or desire relative to the "rise" of a hunch, guess, or hypothesis regarding the factor of difficulty in the situation—namely, μ. Phase *b* consists in a series of more or less haphazard, or possibly routine, observations of the currently contemporary situation, such as may suggest the appropriate hypothesis. The pairs of vertical arrows are intended to represent these minor sets of observations—the first in each case denoting an outward turning of attention, calculated to get some significant cue appositely mirrored or reflected in terms of experience, and the second denoting this latter aspect of the process.

The space R represents a movement of Rational Induction, guided or controlled by the hypothesis, denoted by M_1, as a sort of "trial divisor" for the situation in its contemporary phase. Hence here, also, the arrows, representing acts or processes of observation, are vertical in direction; for the agent cannot now go back to the moment of σ contemporary with S, which is now past. Ra represents a tension relative to securing a factual substantiation of M_1 as M_2. We here distinguish the two arrows of each pair as *b* and *c*, to call particular attention to the items of the movement R, which they respectively denote. The agent reacts to the concept M_1 with a deductive reaction: "If this is

a case of μ (say typhoid fever), I should find such or such a fact here"; and he turns his attention (or his receptors) to the situation, to determine whether such a fact is there, to be reflected back in terms of experience. The arrow c indicates the process of finding—or not finding—the deductively suggested fact. M_2 indicates that the hypothesis has now apparently been substantiated, and that the agent is now ready to proceed more directly towards the intended consummation of his act. In case the first hypothesis has not been found to be true or apposite to the situation, it must be rejected and a new one tried out—a process not represented in the diagram.

The hypothetical form of statement, as just used, does not serve to "prove" the appositeness of M_1 to μ, the problematic factor of σ, but only to direct observation to details suggested by M_1. It is in this movement, R, that Mill's Inductive Methods apply. These are best stated in hypothetical form, to serve as directions for observational operations relative to a discovery or identification of causes, as does the above-named use of this form, for purposes of diagnosis. Thus a given Situation, σ, in a case calling for use of Mill's Methods, is a fact regarded as an effect of some concurrent or antecedent fact as cause. Here the effort is to isolate, among a number of known concomitant or antecedent facts—μ_1, μ_2, μ_3, etc., some particular fact, μ, that may finally be represented in experience as M_2. The Method of Agreement, for example, serves to confirm, among the ideas of detected possible causes, one which may be more certainly used as a trial hypothesis, M_1, for identification of its reference as possibly the real cause. Thus we may state the Method of Agreement, keeping in mind that our M in such a case would be an idea of μ as the cause. "If two or more cases of μ as known consequent have instances of σ regularly associated with them as concomitant or antecedent, then μ may be causally related to σ as effect. On further observation, thus directed, I find that μ is thus regularly associated with σ. Therefore μ as effect *may be* causally related to σ—a cue to be further pursued, by the use of others of Mill's Methods, or by still others."

The reason we have thus proceeded regressively, from conse-

quent to antecedent, is that this is the only safe order to be followed. If a person has malaria, we can safely reason that he has been bitten by a certain kind of mosquito; but if we know that a person has been bitten by this kind of mosquito, or even by the same one, we cannot safely infer that he will have malaria. If we know our field of operations, we can safely reason from facts taken as effects to facts taken as causes; but not in the opposite direction. Hence it is that the experimenter or problem-solver needs to know in advance what sort of consequence or product he will prefer to his current problematic situation. For, knowing this, he can reason backwards to the sorts of causes that may be used to produce it; and then, controlling these causes to the appointed end, he can achieve the desired effect—provided he knows sufficiently well the medium within which he is working.

The space T represents a return to the level of familiar, habitized, or skilled activity—though one might say of a higher level; and the symbol itself represents the movement as being Technological—an application of already learned skills in dealing with the difficulty, as now isolated or defined. The arrows represent processes of attention to the still current situation, in observing the progress of the consummation under way. The symbols *a a* represent an organismic tension relative to the consummation under way, and *b b* represent processes of attention to the work going on. These tensions have actually been persistent—continuously or intermittently—throughout the entire activity from the moment S, and are in fact what has given psycho-logical continuity to the act as a whole. The tensions denoted in the diagram as Ea and Ra are secondary tensions, supervening upon the one continuous pattern of tension here denoted as Ta. The moment P is the moment of final consummation—the Product of the entire act. It is worth while to notice that the consummation of a technological act constitutes a *supplantation* of the difficulty upon which it started, by a Product of the act, which is more desirable than the Situation with which it started. For example, a doctor, in "restoring a patient to health," does not merely stay or kill his patient's

disease; he helps his patient to supplant disease by health. A successful scientist supplants ignorance or error regarding some sort of fact by a public statement of knowledge concerning it, including a description of how he attained to his knowledge.

Now let us consider the symbols S, M, and P, as representing the termini (Terms) of the logical divisions of the act—at the same time noting their fitness for this function. S denotes the reflected Situation, σ, or Stimulus to the act, as its Minor Term; surely a problematic situation is of minor esteem in the thought of the agent, as compared with his hoped-for consummation, or Major Term. It constitutes at once the concluding terminus of the preceding period of complacency, and the initial terminus of the act as a whole. The positions of M_1, M_2, as the rational mean or means by which the consummation is attained, well entitle it to the designation, "Middle Term." And P, as denoting the Product, or result produced by the act, is similarly fitted for denoting the Major Term, or Terminus, of the act, as being the consummation towards which the entire act has been directed.

Our diagram is a rather symmetrical affair, and in this respect may misrepresent a particular reflective act. The movement E may require only a moment of time, while R may require even hours, or days, or years, as in a case of research to discover the precise nature of cancer; and so may the movement T, as in a case of building a dam or a cathedral. Both of these last movements may require the conception, setting up, and working out of one or more experiments, incidental to the whole act; and these may be so significant in their findings as to constitute independent experiments. The case has been presented as that of a single experimenter; but the diagram—and the formula that follows—apply just as well in representing the work of a group of men working coöperatively upon a single problem.

It has been assumed throughout this discussion that the problematic situation was external to the agent—though the presentation would apply equally well to one within him. Thus the terms S, M, and P do not denote the object matters themselves (σ, μ, π) with which the agent is dealing. They denote these

as "reflected"—perceptually or conceptually—into the medium of experience, or in terms of experience, as semantic substitutes for the facts under observation. Hence the logic under discussion is well described, pictorially, as the "logic of reflection"; or, to put it in another way, the total process is thus well denoted as "reflective thinking." The words "reflected," "reflection," and "reflective" are here no mere puns, as one might at first be inclined to suppose. Neither are they so loosely or metaphysically employed as in a statement to the effect that in such a process "thought is here 'bent' upon the solution of a problem." When the words are taken seriously, in the really apposite but somewhat pictorial sense just stated, they are well calculated to emphasize the need of *objectivity* in scientific endeavor.

VII

We have seen that the reflective logic can be represented diagrammatically. We shall now see that it can be also represented symbolically, much after the manner of categorical syllogism. Instead of the mere dash employed in the latter, to symbolize a copula, we shall employ an arrow. This implies that one term means, or points to, a second, as an appropriate processal consequence. Moreover, we shall employ an initial propositional form, of interrogative quality, to indicate the problem that is to be solved. First, we shall thus present the symbolical form of the categorical syllogism, and then that of the reflective act.

1. Let us thus represent the famous syllogism regarding Socrates. The problem here is, "Let us see whether or not—or how—we can make the class concept 'mortal' apply inevitably to the case of Socrates. Can we, or can we not? What Middle Term shall we have to use in order to do this?"

$$\frac{\mathrm{S}\text{——}\mathrm{P}\ (?)}{\mathrm{M}\text{——}\mathrm{P}}$$

$$\frac{\mathrm{S}\text{——}\mathrm{M}}{\therefore\ \mathrm{S}\text{——}\mathrm{P}}$$

If Aristotle had been trying to prove the mortality of Socrates—who had already long been dead—he would have put his problem and argument in this form:

$$\frac{S——P\ (?)}{\begin{array}{l}S——M\\ M——P\end{array}}$$
$$\therefore\ S——P$$

But it was not Socrates that Aristotle was here interested in; it was the problem of how he could use his concept of mortality for the purpose of classifying or putting things in their place. His thought was not a forward, but a backward-looking process. He was proving or testing the efficacy of his Major Term rather than trying to prove anything about Socrates. Moreover, his "thought" is not a continuously logical movement, in his own Figure of this syllogism, as is that of the reflective act—as our formula will show, equally well with the diagram.

2. Let us suppose the case of a physician, called to see a patient. His problem is, "Can I make this case of illness mean a case of health? How can I help this patient to supplant illness with health? I trust I have the skills and medicines for doing this; but, first, what concept of his illness, as a Middle Term, must I have as a stimulus to release the appropriate skills upon him?" We can symbolize his complete successful act with his patient, in two slightly different forms. It is to be noted that both of these forms exhibit a complete logical continuity from term to term, as in the Aristotelian Fourth Figure, but in reverse order, because the "premises" have been transposed.

$$\frac{S——\rightarrow P\ (?)}{\begin{array}{l}S——\rightarrow M_1\\ M_1——\rightarrow M_2\\ M_2——\rightarrow P\end{array}}$$
$$\therefore\ S——\rightarrow P$$

$$\frac{S——\rightarrow P\ (?)}{\begin{array}{l}S——\rightarrow M_1\\ \quad\searrow\\ M_2——\rightarrow P\end{array}}$$
$$\therefore\ S——\rightarrow P$$

Keeping in mind this second formula, and recalling our diagram, we can now add a detail of improvement in this latter. Recall also what has been said about the organismic tension, or desire, that supports the reflective act and gives it logical continuity. We may now think of P as being written with S (S → P), and S with P (S → P). And we may think of the first combination as being written with the numerical subscript *1* after each symbol, and the second combination, with the subscript *2*; and we may think of the first combination as being followed by an interrogation point:

$$S_1 \longrightarrow P_1 \quad (?), \qquad S_2 \longrightarrow P_2$$

In the first combination, S_1 represents the more or less confused (incomplete) percept of the problematic situation, while P_1 represents the concept of the consummation, in anticipatory form. In the second, S_2 represents the concept of the problematic situation, in memorial form, while P_2 represents the percept of the consummation as achieved.

VIII

As previously stated, the Aristotelian terms, "extension" and "distribution," with appropriate reinterpretation, are serviceable in the Deweyan logic. By "extension," in the Aristotelian logic, is meant the relative number of individuals constituting the "classes" named by the terms of a proposition. Thus the predicate term of a proposition regularly has at least equal, and generally a greater, extension than has the subject. Hence the Major Term of a categorical syllogism, as predicate of the conclusion, regularly has greater extension than has the subject, or Minor Term. The same sort of extensional difference, but with a difference of meaning, is to be found in the Minor and Major Terms of the Deweyan logic. Here, in the affirmative or constructive solution of any humanly legitimate problem, the Product, or Major Term, exhibits a greater degree of wholeness than does the Problematic Situation, or Minor Term. The solution constitutes a matter of rational and technical growth in the solver himself; and public communication of it makes possible

a still further extension of abilities, services, powers, life, etc., for humanity as a whole, relative to the kind of situation that was encountered. The extension is both spatial and temporal, or spatio-temporal, in its range.

The conception and solution of any problem, as that of a Napoleon seeking personal fame, power, wealth, or prestige, by arriving at a consummation that has less extension for human life than the situation upon which he worked, as by a slaying or other restriction or diminution of human life, is one whose Major Term has less extension than its Minor. A social-economic-political order that makes its problems those of enriching the controllers of its capital resources by limiting the lives of their many times outnumbering laborers and consumers, and their children, is thus in like manner shown to be engaged in raising and solving humanly illegitimate problems. The Deweyan logic has hithterto found no use for the concept of extension, but here we can discern that the concept may well serve as a significant criterion for the defining and evaluating of human problems, even before an attack is made upon them.

The term "distribution," too, can be appropriated for use in the Deweyan logic—also with a change of meaning. By "distribution," or saying that a Term is "distributed," the traditional logician means that the Term is used in its full extension—either affirmatively (inclusively), by saying "All," or negatively (exclusively), by saying "No" or "not": "*All* men," "*No* men," "Some men are *not* wise," etc. Fallacies of distribution, relative to the Terms involved, are denominated, respectively, as those of the "illicit Minor," "undistributed Middle," and "illicit Major"; and it makes no difference whether the Middle is distributed in its first or second appearance—in the minor or in the major premise. Rarely, it may be distributed in both.

In the solution of a single reflective problem, in the Deweyan logic, it is the second appearance of the Middle Term, M_2, that is the more widely distributed; and here the expression means that the Term is better certified or substantiated, for the purpose under way. Affirmatively, the Deweyan logician will not characteristically say "All," but will perhaps generally restrict his

statement to statistical approximations relative to enumeration, sampling, or correlation. Thus he will have to regard his Minor Term, S, as but a fair sample of a space-time class of difficulties: his Middle Term, M_2, as a statistically reliable cue or instrumentality for the solution of further difficulties of that kind; and his Major Term, P, as a statistically reliable outcome of using his Middle Term in the techniques found applicable to it. Thus the Deweyan logic is wholly extensional, implying the relative individuality or uniqueness of each objective relatum (σ, μ, or π) of a given reflective act, and of each reflective act upon a given kind of problem. In a space-time, processal, and evolving world, this is what we should naturally expect. Only negative propositions can generally be absolute, and their function is to clear the way and prompt to a new start, that may result in a positive relatum reflected as Product or Major Term.

The "truth" that men arrive at, however, coherent or consistent their logical techniques, is relative to the tenability of the postulates from which they take off—or of the objectives towards which they direct their thought and action. For a species, like humanity, which inherits from its natural source and constitution an impulsion to continuance of existence, and whose temporal dimension is incomprehensibly more significant than any of its spatial dimensions or dispersions, the postulated objective of a continuance of its life or existence is the most significant postulate about its own nature, that can be conceived.

IX

I think that some may raise the cry "Psychologismus!" at this presentation, especially at the part that organismic tensions, or desires, have been represented as playing, in giving logical continuity to a thought process, whether Aristotelian or Deweyan. But if they will place themselves in the position of the working legalist or jurist, who make a parade of their use of the Aristotelian logic, but blink their use of the Deweyan, I believe they will be compelled to admit the fact somewhat as I have described it. It would be interesting to have a frank statement of such men, of just precisely what their real adjectives are. These

would quite certainly not be stated in terms of "Constitution," as a set of postulates that they desired to preserve, but in terms of intended future effects, far beyond the briefs or decisions that they write.

Logic—or the employer of logic—does not work with "pure concepts," as with so many pith balls in a vacuum tube, bereft of all desires or interests. It is conceptually integrated desires that prompt the efforts at logical behavior, and that direct, promote, and support them. And it is concepts of ends to be achieved, not concepts of things already done, that do this work. Neither does knowledge as such constitute the final end of effort, but only a set of potential Middle Terms for further effort. That is why knowledge that makes no difference is not actual knowledge. It cannot constitute a Middle Term unless there be conceived a consummation or Major Term, to be achieved as a consequence of its use. The Deweyan logic could not be truly an instrumental logic, without a recognition of this fact.

I think that we can now put the logics of Aristotle and Mill in their places, each as a part-logic, or hypo-logic, within the larger Deweyan logic, as here extended and completed. The logic of Mill, so far as it goes, is a logic of experimental effort, represented in our diagram by the movement R. Since Mill's day, we have developed the statistical methods of enumeration, sampling, and correlation, which also apply in this movement. The Aristotelian logic, in so far as it is usable in such reflective acts as we have here discussed, applies in the movement T, in the building up of a linguistic structure, say a scientific paper, for publicizing the methods and results of a knowledge quest. Besides this use, it also serves as a technique of criticism, by which such linguistic structures, already published, may in part be evaluated for their consistency.

Finally, seeing how the Deweyan logic lends itself to a logically diagrammatic representation, and how capable it is of assimilating the traditional logical terminology for the purpose of technical description, I think there can be no question but that here is a real logic—or psycho-logic. Perhaps, however, it would

better be called a *practic*, seeing that it is an exhibition of the pattern of rationally directed doing or achievement, in which even verbalizing is recognized as a doing.

When at long last our scientists and other technologists shall have recognized themselves as being actually social technologists; when they shall thus have become completely humanized; when each shall have learned to regard himself as a mouthpiece, spokesman, or agent for "all" humanity—a human function relative to the future of the species as a space-time whole; and when they shall have become possessed of an adequate frame of reference for so doing; better still, when such an orientation and the logic of achievement shall have become the possession of every cultured person;—we may then hope to have a culture and a civilization really worthy of creatures possessing the potentially high intelligence and rationality of man.

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The Rôle of the Observer

BY

NORBERT WIENER

THE distinction between psychology, logic, and epistemology is a commonplace. The first treats of experience as an act, experience in its relation to the individual observer. The second concerns itself with the internal marks by which truth may be distinguished from error, and in so far as it deals with experience, has to do with some sort of validity of the experience as evidence of a truth. The third discusses in a general way all the elements—observer, object, immediate presentation—which enter into the experience.

There is clearly a rough practical distinction between these three ways of regarding experience. The interests of an investigator of colour vision are certainly different from those of an investigator of mathematical technique, and both are different from those of a speculator in the theory of knowledge. However, the assertion that psychology and logic are two different sciences may mean something much deeper. There are those who make the distinction absolute, and who assert dogmatically that the logical value of a proposition is entirely unconnected with the relation of this proposition to any observer. The Platonist believes in a world of essence, of cleanly defined Ideas and cleanly defined propositions concerning these Ideas, into which we may enter as spectators, but never as participants. They are out of time, and time is irrelevant to them.

This is pure dogma, and does not check with what we should naively expect. Of course, our experiences must have some ref-

erence outside themselves, in the sense that they cannot be considered as completely closed and isolated. Otherwise there could be no knowledge at all. This by no means asserts that the experience has a reference entirely unaltered by our participation. How hard it is to minimize this participation, is shown by the conventions of the representative arts.

When a dramatist wishes to portray for our amusement or edification some scene of human intercourse, he places it in a room with three sides at oblique angles to one another, with an abundance of doors in utterly abnormal positions, and an entirely unusual illumination. The actors speak the most personal soliloquies, the most confidential whispers, and the best-mannered drawing-room conversation, in tones that carry a hundred feet. A dinner-table will have one empty side, so that everyone may sit facing the audience.

We are used to these conventions, and they do not trouble us. To a first approximation, they are unimportant. This is largely due to the technical skill of the dramatist, who deliberately avoids shocking us by attempts to express the dramatically inexpressible. The conventions of the drama do not interfere with his message because he has the sense not to try to impart any message with which they would interfere. Nevertheless, when the dramatist invades recent history, as is the case with Drinkwater's Lincoln, his dramatic groupings and the exigencies of the stage must conflict with the recollections of contemporaries. Small as his alterations may be, they are real, and important for some purposes.

What is true of the playwright and actor is true of the painter, the sculptor, the author. To discern this, we need not go back to the historical tableaux of David or the be-togaed quasi-Roman busts of his contemporaries. Ruskin has familiarized all of us with the limitations of the painters' palette as against nature. The older painters, by their very painstaking attention to detail, presupposed an eye deliberately searching their picture from spot to spot, and forewent some of the truth of the immediate impression. The impressionists, in declaring the new truth, prevented themselves from expressing the old one. If the

sculptor represents eyes and hair true to their form, his representation belies their appearance; if true to their appearance, it belies their form.

It is however in literature and in the kindred art of illustration that the observer intrudes himself most insidiously. When I was a boy, I possessed a copy of "Treasure Island" with an illustration which troubled me not a little. It showed the hero being chased through the rigging of the ship by one of the mutineers. The point of view from which it was supposed to be seen was about ten feet to the side of one of the masts. "This picture," I said to myself, "represents the scene as somebody sees it. Now, it is explicitly stated that only the two characters shown in the picture are aboard. Moreover, if a third person were aboard and were to see the scene as it is represented, there would be no place for him to stand." In literature, the author also constitutes an extra character, who sees the invisible and divines the unknowable. The more "psychological" a novel is, the more this extra character intrudes.

In psychological works of the imagination, and above all in that great work of the imagination known as psychoanalysis, the author can only reveal by falsifying. It is his task to bring the subconscious motives of his characters to light, but a subconscious motive brought to light is no longer a subconscious motive. It has an outline and a body which are foreign to its original nature. Without doubt the methods of psychoanalysis have a real validity of some sort. Equally without doubt, this validity does not consist in merely tearing aside a veil behind which our original unconscious motives remain unaltered.

Psychoanalysis is not the only branch of medicine in which the examiner has at his disposal only methods which are to some degree destructive. In an abdominal emergency, the surgeon may have to perform an exploratory laparotomy, but an exploratory laparotomy is itself a somewhat drastic surgical procedure. A proper examination of the eye demands the use of mydriatics, which however have blinded many a glaucomatous eye. The physician learns his trade on a dead, preserved cadaver, which death and the use of fixatives have altered in

texture, form, and chemical composition. He can only examine the deeper tissues by the removal of those more superficial. In his histology, he can only discern microscopic structure by the use of dyes which tend to alter microscopic structure. His physiological experiments on dogs often involve the alteration of the course of a salivary duct or the formation of a fistulous entrance to the stomach, or some change more radical still.

These difficulties, of which the biologist is very well conscious, were long strange to the physicist. For centuries it did not seem that the inspection of a physical experiment was in any way relevant to its outcome. The effect of light on ordinary gross mechanical occurrences is so slight that for long it lay far below the threshold of accuracy of our measurements. Nevertheless, with the gradual improvement of scientific technique, the time came when light was found to exert a pressure. This pressure actually alters the mechanical phenomena observed, but it might be thought and was at first thought that any such alteration may be reduced below any desired level by decreasing the intensity of the illumination.

Modern physics has found that a reduction of the intensity of illumination does not furnish a practical way of minimizing its effects on the phenomena observed. Light is radiated in quanta, in units, the size of which bears a relation to the frequency of the light, but not to its intensity. It is impossible to observe an occurrence unless we illuminate it by at least one quantum of light, but the effect of this quantum is definite, and cannot be reduced save by replacing the source of illumination by one of lower wave-length.

We are now brought up against a dilemma which leads to Heisenberg's famous principle of indetermination. If we examine the position of a particle with light of low frequency, our microscope will have a relatively low power of resolution: that is, it will be impossible to determine the position of the particle with any high degree of accuracy. On the other hand, if we examine the particle by light of high frequency, we shall give the particle such an impulse as to mask its original momentum very completely. Every gain in the accuracy with which we deter-

mine the position of a particle is a loss in the accuracy with which we determine its momentum.

One might suppose that it is still possible to maintain that a particle such as an electron still has a definite momentum and a definite position, whether we can measure them simultaneously or not, and that there are precise laws of motion into which this position and this momentum enter. Von Neumann has shown that this is not the case, and that the indeterminacy of the world is genuine and fundamental. There are no clean-cut laws of motion which enable us to predict the momentum and position of the world at future times in any precise way in terms of any observable data whatever at the present time. In other words, while it is possible to give an account of the world in terms of our observations which themselves disturb our world, this account has only statistical validity, and cannot be brought closer to precision by any chain of observations.

Thus physics, the most exact of all sciences, has had to have a thorough logical housecleaning. We no longer conceive the laws of physics to apply to some mystical world of reality behind our observations and instruments: they merely constitute an intelligible statement of the manner in which our observations and the readings of our instruments hang together. About any proposition of physics, we must ask: does it enable us to predict the result of an actual or possible experiment. If it does, it stands or falls with this experiment; if not, it has no meaning whatever. Physics is merely a coherent way of describing the readings of physical instruments.

There is no reason why a similar criterion should not be applied to all branches of knowledge. Biology should be an account of the outcome of dissections, physiological experiments, and observations of the behavior of animals. Psychology should be a reasoned history of introspections and observations of behavior, which will allow us to fit in new observations and introspections. Mathematics should be an account of theorems and their recognized criteria of truth or falsity, which will allow us to place new theorems in this respect. Whatever view we have of the "realities" underlying our introspections and experi-

ments and mathematical truths is quite secondary: any proposition which cannot be translated into a statement concerning the observable is nugatory. It is perfectly conceivable that we shall be led back to the same relations between experiments and observations that we are supposed to obtain at present, but it is equally conceivable that we are being misled by a preconceived notion of "reality" into ignoring a much better correlation between our experiments than we have yet suspected.

Let us apply our new and more stringent logical conscience to the so-called "universals," the Platonic Ideas. It does not take much sophistication to recognize that the universals of science—let us say, "evolution" or "mathematical function"—have a definite history. "Evolution" does not mean quite the same thing to us that it meant to Charles Darwin, and it certainly did not mean the same thing to Charles Darwin that it meant to Erasmus Darwin. It is possible to envisage this progressive alteration in a universal as the gradual rendering explicit of what was already present *in esse*, or in the replacement of one universal by another through successive stages. Let it be noted, either of these views constitutes a prejudging of the logical situation by a reference to a postulated real world behind the observations and has no legitimate scientific status.

The only universals with which the logician has any real concern are the universals as they actually appear on an inventory of our knowledge. These universals, unlike those of Plato, have various degrees of vagueness and mutability. Let us consider as an example the notion of a series in mathematics. Euler made a plentiful use of series, generally with ultimate results capable (at least in many cases) of a correct interpretation, but with a total neglect of what we should now consider the most elementary requirements of mathematical rigor. Convergent series, series summable in the sense of Abel, asymptotic series, all appear mixed together, without the slightest semblance of discrimination. We may say that Euler here applies the theory of convergence, there that of summability, which is a manifest anachronism; or that his work, not being mathematically rigorous, consists of false propositions, or of no propositions at all, which

is an even more manifest piece of pedantry. The actual facts are quite simple. Until the last quarter of the nineteenth century, mathematical science did not regain the degree of mathematical rigor demanded by the Greeks, and Euler was the last man of whom rigor could be expected. There is too much arduous pioneer work on the frontiers of science to permit all the niceties of civilized intercourse; and in the middle of the eighteenth century, scientific as well as colonial frontiers were in a state of rapid recession as a result of the efforts of hardy pioneers. Euler's work was great and crude. His concepts and his proofs were powerful but not clean-cut. We may neither deny his achievement nor read into it the subtleties of later generations.

It might be maintained with some show of plausibility that the heroic age of mathematics is over, that we have devoted the effort of two generations to the improvement of our logic, and that the real timeless universals of elementary mathematics have at last been uncovered. The relative recency of Zermelo's doubts as to the multiplicative axiom, of Russell's paradoxes and of the theory of types by which he avoids them, and of the denunciation of the law of the excluded middle by Brouwer, all tell against this supposition. Even more telling is some of the recent work of Kurt Gödel. Gödel shows that in any mathematical system there are propositions, the truth or falsity of which can only be determined by a reference outside the system. That is, with the best will in the world to determine a mathematical system categorically, in such a way that its propositions can only have one definite meaning, we can only put off the question of its categoricity to that of some larger, more inclusive system. At some stage, we always run back to that which has no adequate mathematical definition. In every actual mathematical system, there are always details which (as far as we know) may be defined in two or more ways, and an imperfect decision between these ways may precipitate us into new logical paradoxes such as those of Russell. No matter how far we go, our logic will always appear Eulerian to some possible scholar of the future.

Thus even in mathematics, precise Platonic Ideas and precise Platonic Truth represent rather asymptotic ideals than anything directly accessible and significant to us. Any really useful logic must concern itself with Ideas with a fringe of vagueness and a Truth that is a matter of degree. A logic which ignores the actual history of ideas and the limitations of human faculty is a logic *in vacuo*, and is useless. The distinction between logic, psychology, and epistemology cannot be made absolute.

In science, two opposite motives are always manifesting themselves: the urge for generality and the urge for concreteness. Mathematics is at the same time in the process of extending its stock of concrete theorems, and of proceeding to more general theories. The urge for the concrete is perhaps most intense in history, economics, and the other human sciences, and above all in psychology. In history, besides the great general principles which govern all human affairs, we have a definite need to know the intimate details of various particular occurrences. An economic theory of depressions is incomplete without a special account of the depression of 1929–?. The psychologist must be a master of the very difficult art of disclosing the concrete content of the mind, of introspection.

The very fact that this art is difficult and must be learned should excite in us a doubt as to whether it is a mere process of unveiling in unaltered form what is already present. Introspection throws immediate light on the content of the mind when thrown into an introspective attitude:—when we deliberately refuse to generalize, even to the extent which is normal in everyday existence, and cast ourselves into a state of the greatest possible passivity. The example of quantum theory should make us most sceptical and unready to take the picture we thus obtain of the content of the mind as representative of what is in the mind under more normal circumstances. Just as universals should only be taken to be definite in so far as they have been through a process of definition, so the data of introspection are concrete only in so far as they have been rendered concrete by a deliberate process.

To define a universal is to render it definite in meaning at any

cost to its definiteness of localisability in time. To place a sense-datum in our introspection is to orient it as precisely as possible in the time-scale of our experience, with so much cost to its logical definition that it becomes a mere "that" instead of a "what." Neither of these processes is in fact capable of being carried out to perfection, but they are fundamentally incompatible, and every improvement in the one is at the expense of our ability to apply the other to the same situation. They have much the same sort of duality as observation by high-frequency light and observation by low-frequency light, as observation disclosing position and observation disclosing momentum. Under normal conditions, the content of our mind is defined in meaning, but imperfectly defined; it is localizable in time, but only to a limited extent. It is only under exceptional circumstances that we either attempt to view things *sub specie aeternitatis*, or try to seize the moment in all its immediacy. Rationalism and empiricism are alike accounts of asymptotic modes of thought toward which we tend in one mood or another, rather than adequate epistemologies of our normal experience.

The logic of our normal thinking, in which universals exist, but have no perfectly clear-cut outlines, and in which the growth of universals is real, and not a mere fictitious account of our perception of eternal verities, is inductive logic. Deductive logic and the canons of pure introspection only exist as ideal limiting cases of inductive logic. The practicing mathematician knows very well that mathematics as a living investigation is inductive and experimental, whatever it may be when stuffed and mounted in text-books. When I want an auxiliary function to do a definite job, I try one after another, finding the first too big here, the second too small there, until by grace of luck and a familiarity with the habits of the species, I come on an exact fit. Nine-tenths of the possibilities are eliminated on the basis of a general feeling for the situation before it comes to a matter of any real deductive logic whatever. The tenth suggestion slips into place in a way which convinces an old hand that there is something in it—it resolves the difficulties at just the right points, but not so readily as to excite suspicions of a

sheer blunder. Once the key will go into the lock, and the bolt begins to show signs of turning, it is a matter of mere file-work and oil to get a perfect fit. The final deductive finish is important for a pretty job, but may be left in the hands of any competent apprentice.

The philosophy of Hume furnishes the dreadful example of what happens to an empiricism which seeks its fundamental reality in the fugitive sense-data of immediate experience. If the raw stuff of our experience does not contain something of a universal nature, no manipulation can ever evoke anything which might even be mistaken for a universal. What Hume has parted asunder, let no God join.

Science is the explanation of process. It is neither possible under a rationalism, which does not recognize the reality of process, nor under an empiricism, which does not recognize the reality of explanation. The problem of the justification of induction, and of the formation of a real inductive logic, which has been one of the eternal scandals of philosophy, arises from the attempt to fit induction into a system originally conceived without reference to induction, in which inductive knowledge is derivative instead of primary. The difficulty is the same as that which we incur when we try to force quantum theory into the frame of classical mechanics.

In Kantian terminology, inductive knowledge is synthetic, for it gives us actual new information. It is certainly at least in part *a posteriori*, but the mind does not receive it purely passively, so that it may be said to contain an *a priori* element. Deductive knowledge is analytic, for the simple reason that no perfect universal has been defined until all possible questions concerning it have been answered, and are consequently contained in its definition.

A particular case of induction which contains much that is typical of more general cases, and merits detailed discussion, is given by periodogram analysis. Periodogram analysis is a method used when it is desired to throw light on the irregular changes of a measurable quantity, such as the air temperature at a particular observing station, and to uncover hidden periodic-

ities. It will be seen without much explanation that the direct record of the temperature as a function of the time is very hard to decipher, and is of very little direct use. It is desirable to give a separate analysis of the components of this phenomena, correlating an amplitude with the period of each component. Naturally, our data only run for a finite interval of time.

The methods of resolving the fluctuations over a finite time-interval into components have what we call a finite resolving power. To distinguish two hidden periods differing in frequency by a quantity a, we need a record running for a length of time inversely proportional to a. If we are dealing with a phenomenon whose general nature varies only slowly, we accordingly take the longest period possible for the basis of our harmonic analysis, and go back as far as our data are reliable.

This is not the right way to handle data subject to profound outside influences. A run of economic data since the onset of the depression may show quite different periods from a run between the war and the depression, and these may again differ from those of data anterior to the war. Accordingly, while very long runs may be of interest to a philosopher of history viewing his subject from the point of view of eternity, they are valueless to the person with normal human interests. We have not yet come to the point where the proper view of human history is that suitable to an observer of the vicissitudes of a population of fruit flies. In other words, periodograms that are historically significant have an intrinsically limited resolving power.

This fact casts a considerable amount of light on the prospects of the extreme mathematical school of economists. These aspire to give refined mathematical methods the same rôle in economics that they already play in physics. The example of the periodogram at least suggests that the humanly important phenomena of economics may be capable of no very precise mathematical definition. The well-trained scientist finds something morally abhorrent in writing down five figures when only three have a meaning. The use of delicate mathematical methods on a very coarse subject-matter is tainted with the same sort of dishonesty. This school of Pareto seems to be tarred with this brush.

It is not merely in the matter of periodograms that the historian finds himself in danger of falling between two stools. The mere unreflective narrator of events does not fulfill the complete function of the historian, nor does the philosopher of history on such a vast scale that individual human lives and motives lose all significance. The intellectual historian must think in terms of such generalizations as church and nation, democracy and fascism, constitution and throne, but he must be aware of the continually changing content of his generalizations. Just where to drop the urge to generalize, and where to obey it, just how much to grant to narration and how much to explanation—this is the great art of the historian, and is only to be learned by practicing it. It is not easy to place an individual occurrence on the canvas of history without so subordinating it that it loses significance.

The philosophical attitude expressed in the present paper is derived from an attempt to apply generally the same maxims that have proved fruitful in the development of quantum theory. These maxims lead to the following propositions, which may be called "postulates" in a very loose sense:

(1) Science is an explanation of process in time;

(2) The act of explanation and of the development of concepts actually consumes a certain minimum of time, and involves a reference to a certain minimum of time;

(3) No concept nor theory is well-defined except in so far as it has been through an actual process of definition;

(4) The time consumed in attaining to a certain degree of definition, as well as the time referred to in a definition of this degree of preciseness, is of an order of magnitude roughly proportional to the degree of precision attained.

I have made no attempt to apply an actual quantum-theoretic account of the nature of life and of the self. In this respect, my method is fundamentally different from that followed by Professor J. B. S. Haldane.[1] He starts with a theory of the mind as a highly degenerate quantum-theoretic system, and

[1] Quantum Mechanics as a Basis for Philosophy. *Philosophy of Science*, Vol. I, No. 1, January, 1934.

proceeds to a theory of universals which leads him to consequences much the same as those of the present paper. His conjecture is a powerful piece of imaginative work, but it *is* imaginative, and the theory of universals at which he arrives is obtained by the transferrence of a physical theory to a field where, to say the least, it has not yet been tested. On the other hand, in this paper, I transfer the criteria and sanctions of quantum theory to new matter, and not its details. My results are entirely compatible with Haldane's. This may be taken as a partial indication that his view of the nature of the mind is quite possibly more than an interesting fairy tale. The dialectical-materialist-quantum-theoretic view to which he inclines certainly leads him to a logic in which propositions (1)–(4) of this paragraph hold good. On the other hand, even if Haldane's account of life is not found to be tenable, our intrinsic discussion shows that the universals of logic are subject to limitations much like those which he ascribes to them.

In proposition (4) of the last paragraph, we have postulated certain numerical relations between time and accuracy. In these relations, certain scale-constants must occur. If Haldane's view is correct, these quantities must bear a definite relation to c, the velocity of light, and to h, the quantum constant. Haldane's theory is not yet developed to the point of formulating this relation, and of course the present paper does not even give an indication that such a relation exists. If it ever is found, it will constitute a very substantial confirmation of Haldane's views.

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The Quest for a Lost Universe

BY

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I. SCIENCE AND EXPERIENCE

SO MUCH has been written recently about the marvelous developments in mathematical physics, that we have almost forgotten that the amazing progress of science in the twentieth century was made possible as much by the skill and machinery of the instrument maker as by the genius of the theoretical physicist. The two greatest achievements of modern physics, the relativity theory and the quantum theory, are magnificient examples of the power of experimental instruments to disclose new possibilities and new realms of theoretical study.

There is no need to labor the point that modern physics, despite its vast theoretical superstructure, is still an experimental science. It is, however, important to note that the development of instruments represents not merely an increased precision in familiar measurements, but a genuine extension of the field of experience. New opportunities for enjoyment and new means of material production crystallize from the theoretical matrix of science in the form of radical technological inventions. And these, in turn, furnish new fields of research and new instruments of scientific measurement. Not only the theoretical formulations but the very subject matter of science changes in the course of its history.

The customary view of the progress of science as an approach to a complete knowledge of nature finds support in history only

if we ignore the unsuspected and unpredictable novelties which have appeared from time to time in scientific research. It is not yet sixty years since Maxwell's superb theoretical achievements stimulated the wide-spread belief that physics had reached a perfection beyond which lay only detailed refinements of measurements but no innovations of theory or subject matter. And yet, almost simultaneously with Maxwell's work, Geissler was developing a technique for evacuating tubes to a high vacuum by means of which he generated the phenomena leading to Roentgen's discovery of the x-rays. Subsequent studies in x-rays disclosed that Maxwell's theories could not be adequately applied to minute electrical disturbances. This discovery marked a new era in physics. It is a significant and much neglected fact that this era was ushered into history by the invention of a suitable high-vacuum pump.

The history of science abounds in other important cases showing that technological developments are of the utmost significance for theoretical science. Such cases are of twofold significance. In so far as the inventions of technology are the products of scientific knowledge, they testify to nature's fundamental simplicity and limitation; and in so far as they transform material conditions and create new relations between things, they reveal an equally fundamental complexity and infinity in nature. They at once confirm and belie that there is nothing new under the sun.

"Nature" said Isaac Newton, "is pleased with simplicity, and affects not the pomp of superfluous causes." But is it not Newton rather than nature who takes delight in simplicity? We find some confirmation of this conjecture when we recall that this quaint remark occurs not as one of the laws in Newton's *Mathematical Principles of Natural Philosophy*, but as a "Rule of Reasoning."

But Newton's statement is no mere methodological rule. For a method that finds successful application in a subject matter must somehow characterize that subject matter; else how can we explain its applicability? Simplicity certainly characterizes the mathematical laws which, in the hands of Newton, achieved

such splendid systematic coherence. That it must also characterize nature is evident from the fact that for centuries these laws enabled scientists to explain all natural phenomena, and that today they still form the foundation of modern technology. Risking a hyperbole, we might say that our industrial civilization—a creature of Newtonian technology—is living proof that "Nature is pleased with simplicity."

Recent interpreters of science have been profoundly impressed by the disparity between the abstract simplicity of the world of science and the infinite complexity of the world of sense experience. The former, consisting of some uniform material parts in changing spatial relations, is devoid of all that qualitative variety which we discover in the latter. The electrons and waves of the physical universe of science bear not the least semblance to the universe of experience which is inhabited by tables, spectroscopes and scientists, and which physics purports to explain.

It is primarily this paradoxical disparity that prompted the philosophical ventures of Sir James Jeans and Sir Arthur Eddington. But their problem, although they treat it in the setting of modern science, is not peculiar to relativity and quantum physics. It was raised 2300 years ago by Democritus and since then by others whose names form the roster of illustrious philosophers in history.

II. THE CONCEPT OF MATTER

Modern discoveries in mechanics, electrodynamics and optics have introduced new physical concepts and fundamentally altered the relations among the familiar concepts of classical physics. Regarded in their most general aspect, these discoveries marked a transition from the view of the physical world as an enduring substance located in space and time, to the concept of the physical world as a system of metrical relations.

A brief review of the evolution of the concept of matter will disclose two paradoxes of a universe without a material substance. It is the effort to resolve these paradoxes that leads Eddington and Jeans to the rediscovery of substance in a Universe of Mind.

What is matter? Too often this question is regarded as demanding a revelation of some ultimate "nature" of things, and so the answer is sought not in science but in the speculations of metaphysics. But it is clear that the question is answered—as far as we can answer it—in the physical sciences. Indeed, the physical sciences are an attempt to answer this question. What physics is about, is matter.

Physics, however, gives us only a knowledge of relations. Its laws disclose the relations between things, while the question is concerned not with how various kinds and quantities of matter are related but with what matter is. This distinction—which underlies much of Eddington's philosophy—is meaningless despite the support it finds in an old tradition. The only knowledge of what a thing is, is the knowledge of its relations to other things. What an object is in itself, unrelated to anything else, is unintelligible because an object unrelated to anything is inconceivable. The nature of an object, as distinguished from its relations, cannot be known, not because its nature is hidden from us, but because its relations are its nature.

Scientists were not always aware of this fundamental fact concerning knowledge. And so they divided their knowledge of things into two types: the knowledge of properties, and the knowledge of relations. For example, the properties of matter for Newton were solidity, mass, size, hardness, impenetrability, and inertia; the relations were distance, temporal succession and various kinds of motion. It was generally believed that the properties of matter were entirely unaffected by the relations into which it entered.

With the development of physics, scientists realized that many of these "properties" were not independent traits of matter but manifestations of relations or of the remaining properties. The essential properties of matter were thus reduced to mass, inertia, and size, with three absolute quantitative determinants of material phenomena: mass, length, and time.

The discovery of the theory of relativity, however, eliminated these last vestiges of the concept of material properties. By showing that mass, length and time are not absolute determi-

nants of matter, but are dependent on motion, the relativity theory finally reduced the properties of mass, inertia, and size, to the status of relations.

We are thus confronted by apparently contradictory concepts. Matter no longer has any properties. Does matter still exist? If matter itself is a complex of relations, what is being related? There is motion. But what is it that moves?

Let us look at the situation from another angle. Newton's physics was based on the concept of an empty space and a material substance. The geometrical properties of space and the physical properties of matter gave rise to the universal laws of motion. These and the law of gravitation governed all phenomena. The electrodynamics of the nineteenth century substituted the concept of the field for that of empty space, and merged the geometrical properties of space with some of the physical properties of matter. With the development of electromagnetic theory, the concept of matter independent of the electromagnetic field was gradually abandoned. Finally, in the theory of relativity matter, space, and field are completely merged in the single concept of the space-time manifold. The concept of matter does not obtain separately from the concept of gravitational field but is fused with it: and the same mathematical functions which determine the metrical characteristics of space also determine the physical characteristics of the field.

Here again material substance has lost its identity in a set of functional relations. The mechanical properties in terms of which we originally conceived matter as an identifiable substance are now indistinguishable from the metrical properties of the field. Matter neither moves nor stands still, for as Weyl says, "there is no longer any meaning in speaking of the same position of matter at different times."

There is still another line of development leading to similar results. In the earlier stages of electrodynamics there was a duality between matter and radiation. Matter was an atomic substance; radiation was a continuous energy wave. Maxwell's electromagnetic theory weakened this duality by showing that the inertia of matter depends on its energy. The duality of

matter and radiant energy was further undermined by the Maxwell-Lorentz conception of the electron not as substance foreign to the field but as an "energy-knot." Matter becomes a phase of the field, and is differentiated from the rest of the field only by its high field quantities and electrical densities.

The development of the quantum theory disclosed not only that energy, like matter, is atomic, but also that matter as well as energy has continuous wave-like characteristics. The close connection between matter and radiation was further established by Bohr's discovery that Planck's constant, which formerly was associated only with phenomena of radiation, was also a fundamental characteristic of certain mechanical phenomena within the atom. A similar fusion of optics and mechanics was achieved when Einstein established the velocity of light as the upper limit of mechanical velocities.

Wave mechanics completes this process of unification. Any piece of matter is viewed by it as the center of energy of a group of waves. Similar groups of waves are associated with light and other forms of radiant energy. While physicists are not agreed on the full significance of these waves, many of them favor the view that the equations of propagation of the material waves express not the dynamic relations of matter but the probability of such relations obtaining at any particular place. It is also conjectured that the energy and momentum of material particles are identical respectively with the frequencies and lengths of these waves.

Once again we see matter lose its enduring substantial existence. Its last physical traits, energy and momentum, are revealed as time (frequency) and space (wave-length); while energy and momentum, as well as space and time, become fused into the pervasive space-time continuum. Matter has become no more than the probability of the conjunctions of certain metrical and physical relations.

III. THE UNIVERSE OF MIND

In the modern concept of matter—notes Eddington—our vivid impressions of the environment play no part. The world of

physics, while it can "imitate the actual behavior of the world of familiar experience" is a world which we build up not from the materials of experience but from the symbolic elements of physics.

Eddington regards the worlds of physics and of experience in isolation from each other. But evidently there must be some intimate connection between them. For these changing concepts of matter have altered the face of the earth. The substance theory has filled our days with the hum of mighty engines, built railroads, peopled deserts and erected great cities. The field theory wove a net of copper over the earth and sent the power of heat, light and motion to remote regions. It built the apparatus of wireless communication and caused busy parliaments to legislate concerning the frequencies of electromagnetic waves and the densities of the field.

By isolating the physical world from experience, Eddington raises what we might call the paradox of technique: the concepts of physics are mental constructs devoid of empirical content, and yet as the preceding paragraph has shown, they can be applied to the activities of the world of experience.

Eddington notes another problem which arises from the fact that scientific explanation does not lead us to any "ultimate knowledge." Physics explains matter in terms of stresses and potentials, these are explained in turn, by intervals; then intervals by scales; and finally coming around full circle, scales are explained by the matter which we originally started to explain. This cyclic nature of physics may be called the paradox of explanation. Scientific explanation never leaves the deductive system of scientific concepts; it does not refer these concepts to any irreducible and directly known thing, yet it claims to give us a knowledge of reality.

These paradoxes exist only for those who, like Jeans and Eddington, build their philosophies on the assumption that science has merely formal, conceptual significance. The paradox of explanation is viewed by them as evidence of the unreality of the physical world, while the paradox of technique is regarded as pointing to a common substratum—or "Substance"—of ex-

perience and science which makes possible the interaction between them.

The concept of material substance with observable properties served as such substratum in classical physics. It is possible to show, however, that the qualitative terms such as "hardness" and "solidity" which Newton applied to his atoms did not have the same significance in his theoretical system as they had in common usage. It could be shown that all these terms in his science stood not for qualitative percepts but for metrical and dynamic relations. So interpreted, classical as well as modern physics would involve the paradoxes of Eddington.

Such analysis would, of course, be unhistorical and inconsistent with the spirit of Newton's science. It would ignore the important factor of the scientist's conscious approach to his problems. But it would demonstrate that the objects of science had always been relational complexes, and that their substantial and qualitative features were extraneous metaphysical and psychological addenda.

Indeed, in contrasting the substantial "reality" of classical physics with the "unreal" abstractness of modern science, Eddington and Jeans contrast not two systems of science but two systems of philosophy, namely, contemporary positivism and Cartesian dualism. Positivism can interpret both classical and modern physics relationally, while the philosophy of Descartes could ascribe substantiality to the space-time continuum of the relativity theory just as it ascribed substantiality to the Newtonian atom. In fact, Descartes partially anticipated such a space-time substance when he asserted the identity of matter and extension.

Eddington's resolution of his paradoxes is an attempt to rediscover the substantial universe of Descartes in the world-view of modern science. Science, we are told, dispels the illusion of materiality which colors our world of experience, and philosophy dispels the illusion of reality with which science invests the world of physics. The qualitative world of experience is only an illusion; the physical world is only a symbol. That which is neither symbol nor illusion—but in which both symbol and illusion somehow have their being—is mind or spirit.

This, however, is an anomalous termination of the quest for a substantial universe. Mind is not indubitably what Eddington takes it to be: "the first and most direct thing in our experience." The method of analysis whereby Eddington disproved the substantiality of matter can also disprove the substantiality of mind. It can show that mind is not *in* experience at all, but that it *is* our experience of thoughts and things. In Eddington's philosophy, however, the scientific positivism, which so effectively expelled material substance from the physical world, is forbidden entry into the realm of mind. Here the mind-substance of Cartesian metaphysics still persists: an oasis of "reality" in the sands of human illusion and cosmic symbolism.

IV. THE UNIVERSE OF LIGHT

It will be recalled that this quest for a lost universe of substance was prompted by the extreme disparity between the appearance of things and our scientific description of them. In a certain sense this contrast has been minimized by the rise of wave-mechanics. The protons, and electrons of matter, as well as the photons of the diverse kinds of radiation, are all wave-like in character. Since it is through one of these radiations, light, that we gain most of our acquaintance with things, a physical world conceived as a system of radiations akin to light in its structure, would bear a familiar aspect. Not only the visual, but all other sense perceptions as well, are due to the absorption of radiant energy by the body. Sight, smell, sound, touch, and taste, are but our reception of photon, proton and electron waves. All these forms of sense perception, as well as all things perceivable, are manifestations, in one form or another, of energy waves. Because among these, light is the most familiar, we may generically call them light waves.

It is this "magnificent inclusiveness" of electromagnetic waves that leads Sir William Bragg to say in his latest book, *The Universe of Light:*

"Light, therefore, using the full meaning of the word, transmits energy which is the mainstay of life, and gives to living beings the power of observation; and it is akin to the matter of

which all things animate and inanimate are made. The universe is its sphere of action. We do it no more than justice when we speak of the Universe of Light."

Bragg's book begins a brilliant account of optics by saying that "Light brings us news of the Universe" and concludes with the remark: "We rightly speak of light as constituting the universe."

This conception of the universe as a Universe of Light is one of extraordinary beauty, but the term light may lead to ambiguities since it might be understood in the restricted sense of that small class of radiations by which we receive visual sensations, as well as in the comprehensive sense in which Bragg uses it. To avoid these ambiguities we will use the prosaic but unambiguous expression of a Universe of Energy Waves.

Parapharasing Bragg, we may then say that energy waves are the universe and they are also bearers of news about the universe.

At a recent conference of the American Philosophical Association, Professor Frederick J. E. Woodbridge startled the assembled philosophers with this problem: If light is a messenger bearing news of the universe, do theories of light illuminate the news or the messenger? In other words, if the energy wave comes to us from some object, does physics—which is a study of energy waves—give us knowledge of the object or the wave? For example, when a physicist studies the constitution of a star by means of a spectroscope, his measurements depend on the energy waves in the region immediately surrounding his spectroscope. Applying Woodbridge's question to this case: Assuming that the waves around the spectroscope came from the star, does this physicist acquire knowledge of the waves or of something known as "the star" which emitted the waves?

But the atom in the star which emitted the wave is, according to wave mechanics, the center of energy of a wave group. The star is a conglomeration of such centers of energy. And it is in vain that we search modern books on optics for a knowledge of these waves other than their mode of travel. All that we know about the universe is derived from the equation of propa-

gation of the waves. They may be messengers bearing news of the universe, but we know only the messenger's mode of travel. And as Woodbridge says: "There is considerable difference between the way a messenger goes from one place to another and the message he brings." But what then is the message?

So far, Woodbridge's analysis seems to have brought us to a position analogous to Eddington's. Physics, from Eddington's viewpoint, gives us only a system of pointer readings; from Woodbridge's viewpoint, an account of how waves are propagated. Eddington shows that the equations of propagation are themselves just systems of pointer readings. Can such a system of pointer readings tell us what the universe is? Eddington says no.

Yes, says Woodbridge. Waves of which we know only the mode of propagation bring us two important messages about the universe. 1. If these waves constitute the universe, then all we can know is what we perceive in these waves. "This seems particularly true in the case of light. Whether it travels by way of wave or corpuscle or by way of both together, its message at least, for those who see, is that the visible world is the chief object and source of all their knowledge." Or more generally, the sensible world of experience is the "chief object and source" of all knowledge. 2. The equations which express the way waves travel in the universe, are themselves "determinations or delineations" of what such a universe is. The message that the waves bring to us is physics. They tell us that the universe we inhabit is of such a character that if it is delineated at all it must be delineated in the terms of physics.

The news which the waves bear of the universe may therefore be summed up as follows: the only world we can know is the world of sense experience; what we know about this world is physics.

V. SCIENCE AS TECHNIQUE OF ACTION

Eddington's Universe of Mind and Woodbridge's Universe of Light display the cosmic vista from two widely separated viewpoints. The mixture of Cartesianism and Pythagorianism in

Eddington, is replaced in Woodbridge by a naturalistic metaphysics which recognizes the objective reality of the physical world. We live in a realm of being in which we find that one thing can signify or imply the existence of another thing; in other words, it is a realm in which we discover a logical structure. The physical world is this structure. It is not a "real world" as opposed to an illusory world of experience. It *is* the structure of the world of experience, and the structure of this world is neither more nor less real than our experience of it.

On the other hand, the philosophical writings of Eddington and Jeans are pervaded by the view that the objects of science are mental constructs and that they signify nothing more than the numbers which the experimenter reads on his various instruments. Such interpretation ignores the fact that measure-numbers are not pre-existent determinants of the world, but characterize the outcome of certain operations. The manipulation of instruments whereby measurements are made is inseparably involved in the meaning of the numbers which represent the measurements.

Eddington's view of the concepts of physics as patterns of measure-numbers, or as he calls them "pointer-readings," may be characterized as the arithmetical or Pythagorean interpretation. Because of the fundamental place it assigns to number it views the world of science as a kind of abstract, numerical replica of the world of experience. Consequently it regards the passive seeing of numbers on a scale as the sole empirical content of science.

Opposed to this view is the operational or Heraclitean interpretation for which the physical concept signifies operations performed on some material. It rests the significance of science in the controlled transformation and redistribution of material and regards "pointer-readings" as controlling indices in the manipulation of instruments. Here the empirical basis and content of scientific theory is scientific technique in which the concept is fused in the processes of sensible material.

The twofold aspect of the scientific instrument reflects this convergence of number and act in science. The scientific in-

strument is not merely a scale: it is also a machine. Besides serving as an instrument of measurement, it serves also as an instrument of transformation. The spectroscope does not merely measure the distance between spectral lines, it changes the light-beam whose wave-length it is designed to measure. As machine, the scientific instrument modifies and reconstructs its subject matter: it redistributes the material in a situation and extends the field of perception. As scale it discloses numerical proportions in these situations.

Corresponding to these two inseparable aspects of the instrument, there are two equally inalienable phases of science: explanation and technique. The function of explanation is the construction of deductive systems in which all observable phenomena are expressed as quantitative relations between such scientific objects as space-time, energy, and waves. This is what Eddington calls "world-building." The function of technique, on the other hand, is the transformation and control of material in order to construct certain desired objects and situations.

By ignoring the fundamental rôle of technique in science, Eddington and Jeans have created a spurious isolation of the explanatory or conceptual phases of science. Divorced from technique, that is, from the objects of sensible experience, the concepts of science, as they become more abstract, lose their empirical content. The concept becomes associated not with physical operations but with arithmetic. In relating scientific theory to its empirical foundations, Eddington refers it to the instrument as scale, or number, and not to the instrument as machine or act.

It is inevitable that such analysis should lead to a conception of the physical world as pure number. But interpreted with due regard to the origins of scientific knowledge, the physical world is seen to be neither "pure number" nor "structure" but a method of activity. Extending Professor Woodbridge's remark, we may say that human activity "is the chief object and source of our knowledge." Despite the cyclic nature of scientific explanation, the concepts of science do have reference to something other than sets of pointer-readings. Wave-length may be ex-

pressed in scientific theory as a number, but it is a number which corresponds to some specific activity involving specific instruments. It is this activity that the concept of "wave-length" signifies. Scientific objects, then, do not reveal *things*, but specify a course of action. They do not constitute the universe; they regulate human activity.

If the concepts of science do not disclose the constitution of the universe, but merely furnish a technique of action, can we never know what the universe really is? Our only answer is that those who have eyes can look to see what the universe is. But if they would *know* what they see, they must act. This activity does not constitute the universe but—when expressed in language—it constitutes our knowledge of the universe.

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Pseudo-Scientific Economic Doctrine

BY

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AS A concrete extension of analyses of scientific method and social study already undertaken in the pages of this journal and elsewhere, the present article will endeavor to clarify certain long-standing preconceptions in classical and neo-classical economic doctrine which still persist and which, in the light of 20th century methodology, can be given no other label than that of pseudo-scientific.[1] These preconceptions revolve primarily about the ideas of "value," "cost," "utility," and "price," which ideas have suffered little change to the present day and which even early writers recognized as being vague and ambiguous.[2]

PART I. CLASSICAL COST AND UTILITY THEORY

The two historical value concepts are value-in-use and value-in-exchange, the former leading to utility theory and the latter to cost theory, which the early classical economists espoused.

[1] *Cf.*, Joseph Mayer, "Pseudo-Scientific Method in Economics," *Econometrica*, October, 1933, pp. 418–430; "Toward a Science of Society," *The American Journal of Sociology*, September, 1933, pp. 159–179; "Scientific Method and Social Science," *Philosophy of Science*, July, 1934, pp. 338–350; and "The Techniques, Basic Concepts, and Preconceptions of Science and Their Relation to Social Study," *Philosophy of Science*, October, 1935, pp. 431–483, especially section dealing with economic utility and value, pp. 475–481.

[2] David Ricardo, "On the Principles of Political Economy and Taxation," 1st ed., *John Murray*, London, 1817, p. 5: "From no source do so many errors, and so much difference of opinion in that science [political economy] proceed, as from the vague ideas which are attached to the word value."

Although utility was recognized by the classical writers, it was given a minor rôle, as was scarcity also. In short, classical theory emphasized cost, neglected utility and scarcity, and always assumed freely operating competition.[3] With respect to price, a distinction was drawn between natural (or, later, normal) price and market price, which under freely operating competition, however, were thought to be virtually the same.[4]

Cost was divided chiefly into money cost and subjective cost; the one representing the pecuniary outlays of the business man, the other representing the subjective sacrifices undergone, such as "tired muscles and tedious waiting."[5]

Fallacies Inherent in the Classical Cost Theory of Price Determination

Classical cost theory was, as just indicated, originally based on a distinction between money costs and subjective costs and on certain underlying assumptions; and normal price was thought

[3] *Cf.*, Adam Smith, "Wealth of Nations," *J. F. Dove*, London, 1826, p. 33; Ricardo' *op. cit.*, Ch. I, On Value; and J. H. Hollander, "The Development of Ricardo's Theory of Value," *Quarterly Journal of Economics*, August, 1904, pp. 455–491.

[4] Ricardo, *op. cit.*, 1821 ed., pp. 80, 84, 85. In making this distinction between market and natural price, Ricardo was following Smith.

What Ricardo designated as 'natural' price was called by later economists 'normal' price.

Cf., also, C. W. Macfarlane, "Value and Distribution," *Lippincott*, Philadelphia, 1898, p. 26; also Veblen's criticism, *infra*.

[5] *Cf.*, F. W. Taussig, "Principles of Economics," 3rd ed., *Macmillan*, New York, 1930, vol. I, pp. 168–69. *Cf., also*, Adam Smith, *op. cit.*, p. 37; "Letters of Ricardo to McCulloch," edited by J. H. Hollander, *Macmillan*, New York, 1895, letter of May 2, 1820, p. 65; J. E. Cairnes, "Political Economy," *Macmillan*, London, 1874, pp. 80–81; S. M. Macvane, "Marginal Utility and Value," *Quarterly Journal of Economics*, April, 1893, p. 269. With respect to economic value in general see:—Gustav Cassel, "Theoretische Social-ökonomie, 5th ed., *C. F. Winter*, Leipsic, 1932; Werner Falk, "Das Werturteil," *Borschardt*, Berlin, 1932; Else Frohnhäuser, "Das Werturteil in der Volkswirtschaftslehre," *Oldenbourg*, Munich, 1929; Walton H. Hamilton, "The Place of Value Theory in Economics," *Journal of Political Economy*, March and April, 1918, pp. 217–245, 375–407; Rudolf Kaulla, "Die Geshichtliche Entwicklung der Modernen Werttheorien," *H. Laupp*, Tübingen, 1906; Ludwig von Mises, "Grundprobleme der Nationalökonomie," *G. Fischer*, Jena, 1933; C. and C.-H. Turgeon, "Premières Études, La Valeur d'Après les Économistes Anglais et Français," 3rd ed., *Librairie de la Société du Recueil Sirey*, Paris, 1925; Allyn A. Young, "Some Limitations of the Value Concept," *Quarterly Journal of Economics*, May, 1911, pp. 409–428.

to be determined in the main by competitive costs of production, that is, by the necessary expenditure of labor and capital, the effect of land being regarded as nil. These assumptions are now recognized either as fallacious or as too sweeping, but in the light of them classical cost theory was logical and understandable.[6]

Among the important hypotheses that Adam Smith laid down, the most important was undoubtedly that under "early and rude" conditions goods exchanged according to their embodied labor,—an hypothesis which modern anthropological research has proved to have been without foundation.[7] A better description of "early and rude" conditions indicates: (1) that they were completely inconsistent with any kind of exchange, since in earliest savagery there was nothing to exchange; (2) that later, traditions of forced exchange, tribute, and the dominance of the stronger, just as much as (if not more than) free exchange or barter, appear to have marked the early valuations set upon commodities; (3) that then there followed class differentiation, distinctions of caste, preference, and property, and the setting aside of special holdings for the lords and masters. In short, predatory management, monopoly, and capital seem to have developed hand in hand with the institution of exchange.

The classical exchange-under-a-natural-economy assumption has been widely used in two ways: first, as a belief that the imputed natural-labor measure once existed and that, although overlaid with more complex conditions, it still exists today (Ricardo's view); second, as an ideal state once existent and now gone but possible of attainment again in some future golden age (the view of the socialists). The argument that the embodied-labor measure, even though unreal, may be used as an abstraction in system-building will not hold, since the alleged measure

[6] Raymond T. Bye, "The Nature and Fundamental Elements of Costs," *Quarterly Journal of Economics*, November, 1926, pp. 30–31.

Cf., also, Jacob H. Hollander, "Ricardo's Theory of Value," *Quarterly Journal of Economics*, August, 1904, pp. 455–491; and his "David Ricardo, a Centenary Estimate," *The Johns Hopkins Press*, Baltimore, 1910.

[7] *Cf.*, Adam Smith, "Wealth of Nations," *J. F. Dove*, London, 1826, p. 51; and J. L. Myres, "The Beginnings of Science," in F. S. Marvin's "Science and Civilization," *Oxford University Press*, London, 1923, pp. 10–12.

is clearly fallacious as applied to any real society and can therefore not be legitimately used as a scientific hypothesis.[8]

Smith himself saw that "embodied labor" did not fit the conditions of his day, so he offered as a substitute "commanded labor," an even more subtle fallacy, since it depends upon the former and stands or falls with it. Reproductive commanded labor as a substitute for embodied labor holds in Smith's assumed rude society, where it would be superfluous, and proves fallacious when applied to real economic conditions. The equivalence of the commanded and embodied labor measures in modern society is assumed, not proved, as Ricardo perceived and as Smith and Malthus failed to perceive. . . .[9]

In his early studies, Ricardo followed Smith, except that he refused to be misled by this assumed equivalence between embodied and commanded labor and except that he was apparently interested primarily in practical problems pertaining to wages,

[8] *Cf.*, Jacob H. Hollander, "Economic Theorizing and Scientific Progress," *American Economic Review*, Supplement, March, 1916, pp. 124–139; also, Mayer, "Pseudo-Scientific Method in Economics," *op. cit.*, and "Toward a Science of Society," *op. cit.*

[9] Adam Smith, *op. cit.*, pp. 41, 52–53. *Cf.*, also, Harlan L. McCracken, "Value Theory and Business Cycles," *Falcon Press*, United States, 1933, chapters on 'embodied' and 'commanded' labor; and Hollander, "Ricardo's Theory of Value," *op. cit.*, pp. 461–463. To Hollander belongs the credit of re-establishing the distinction between 'embodied' and 'commanded' labor in modern discussion. See J. R. Commons, "Acknowledgement," *The Review of Economic Statistics*, April, 1923, p. 114. *Cf.*, also, "Letters of Ricardo to Malthus," edited by James Bonar, *Clarendon Press*, Oxford, 1887. Regarding Malthus's use of commanded labor, Ricardo wrote (pp. 236–237): "I still think that the invariability of your measure is the *definition* with which you set out, and not the *conclusion* to which you arrive, by any legitimate argument." *Cf.*, also, *ibid.*, p. 218; and David Ricardo, "Principles," *op. cit.*, 1817 ed., pp. 6–11. Speaking here of Smith's commanded labor theory, Ricardo wrote:

". . . Sometimes he speaks of corn, at other times of labour, as a standard measure; not the quantity of labour bestowed on the production of any object, but the quantity which it can command in the market: as if these were two equivalent expressions, and as if because a man's labour had become doubly efficient, and he could therefore produce twice the quantity of a commodity, he would necessarily receive twice the former quantity in exchange for it.

"If this indeed were true, if the reward of the labourer were always in proportion to what he produced, the quantity of labour bestowed on a commodity, and the quantity of labour which that commodity would purchase, would be equal, and either might accurately measure the variations of other things: but they are not equal."

profits, and the like.[10] Including present and past labor in his doctrine, denying the influence of land, and dismissing certain exceptions as unimportant, he maintained that in general embodied labor remained a proper measure of price.[11] Ricardo was forced by his critics, especially by Malthus, to modify his doctrine as time went on; but, on the other hand, his flexibility of thought won over some of his most powerful antagonists, such as McCulloch and James Mill, who, at the same time, later proved to be less open-minded than he was.

In the debates that followed, Ricardo came to admit, despite the protests of McCulloch and Mill, that the "time" element appeared as an important qualification to a strictly labor-embodied measure; and, after further criticism from Malthus, he gave "elapsed-time" a coördinate place with "quantity of labor" as the measure of price.

Ricardo consistently refused to accept Smith's alternative commanded-labor measure, which Malthus espoused, but in the end he appeared to offer a third alternative for practical purposes, namely, a money measure composed of wages and profits, thus (whether unwittingly or not) perpetuating the confusion between subjective and objective conceptions of cost, which still persists in our own day.[12]

Ricardo's cost doctrine presents a distinct and consistent evo-

[10] Hollander, "Ricardo's Theory of Value," *op. cit.*, pp. 455–491; also his "David Ricardo, a Centenary Estimate"; "Letters of Ricardo to McCulloch," edited by Hollander, *Macmillan*, New York, 1895; "Letters of Ricardo to Trower, 1811–1823," edited by Bonar and Hollander, *Clarendon Press*, Oxford, 1899; and "Letters of Ricardo to Malthus, 1810–1823," edited by Bonar, *Clarendon Press*, Oxford, 1887. Some recent authors appear to disagree with the more generally held point of view regarding Ricardo's primary early interest in practical problems. *Cf.*, E. S. Mason, "Ricardo's notes on Malthus," *Quarterly Journal of Economics*, August, 1928, pp. 684–696 (687–688).

[11] Ricardo, "Principles," *op. cit.*, 1817, p. 16. Also, *Ibid.*, p. 67. This, of course, assumes conditions of freely operating competition as applied to land. Also, *Ibid.*, 1821, pp. 36–41.

[12] *Cf.*, R. Torrens, "Cash Payments with Strictures on Ricardo," *R. Hunter*, London, 1819; and Hollander, "Letters of Ricardo to McCulloch," *op. cit.*, pp. 15–16, Note; pp. 47–48, December 18, 1819; pp. 63–65, May 2, 1820; Letter of August 21, 1823, p. 177. *Cf.*, also, T. R. Malthus, "Principles of Political Economy," *Wells and Lilly*, Boston, 1821; J. R. McCulloch, "The Principles of Political Economy," *Tait*, Edinburgh, 1825; James Mill, "Elements of Political Economy," *Baldwin, Cradock and Joy*, London, 1821, pp. 72–75; "The Works of David Ricardo," edited by McCulloch, *Murray*, London, 1846.

lution, from his early preference for the embodied-labor measure, through his successive admissions of exceptions until he regarded "labor and time" as a more exact description, to his seeming suggestion of money cost as an even more practical alternative. . . .

As a supplement to criticisms made by the present writer elsewhere, it is now possible to discern the evolution of the "alternative" dogma,—commanded labor as a substitute for embodied labor, and, in turn, commanded money as a substitute for either of the other two. Starting with the unreal assumption of "embodied labor" as the measure of price in primitive society, cost theorists suggested "commanded labor" as an alternative in advanced society. Later they suggested "commanded money" as a still better alternative. All three measures were regarded as equivalent, which they would be only under the false primitive-economy presumptions. In a real society they are by no means equivalent, nor is proof ever submitted to show that they might be. The "alternative" fallacy thus remains to plague the economist.[13]

To assert that money costs measure price is wholly superficial, since money costs are themselves prices. Economists have all along been seeking something more fundamental, the real or subjective costs underneath. The "alternative" dogma, whether by the commanded route or (later) by the opportunity-cost and might-have-been route, fails in any sense to reveal these fundamentals.[14]

Rise and Decline of Cost Theory

Smith and Ricardo gave us the best that there is in classical cost theory, so that the doctrinal modifications of their followers need not detain us long. After Ricardo's death in 1823 there

[13] *Cf.*, Mayer, "Pseudo-Scientific Method in Economics," *op. cit.*, pp. 421–425; also, *cf.*, Patten's comment regarding Smith's and Ricardo's position on this score: S. N. Patten, "Dynamic Economics," *University of Pennsylvania*, Philadelphia, 1892, pp. 27–28.

[14] *Cf.*, F. W. Taussig, "Principles of Economics," 3rd ed., *Macmillan*, New York, 1930, vol. I, p. 169; Bye, *op. cit.*, p. 30; Edward S. Mason, "The Doctrine of Comparative Cost," *Quarterly Journal of Economics*, November, 1926, p. 76; and H. J. Davenport, "The Economics of Enterprise," *Macmillan*, New York, 1913, pp. 84–104.

was a refinement of the old fallacies, represented in attempts to make subjective cost equal money expense and in a rigid advocacy of the embodied-labor or the commanded-labor measure of price. The "perfected" system saw its most forceful expression through John Stuart Mill (1803–1873), after which it declined as a result of numerous critical attacks.[15]

The younger Mill really represented a transitional stage in cost theory, for he made damaging admissions regarding the older doctrine, attempted to gloss over weak spots and to rephrase ideas to fit the contemporary mood, and continued the confusion between subjective and money costs.[16] Senior resurrected Ricardo's emphasis on "time," calling it "abstinence"; Cairnes insisted that real and money costs must be kept separated; and

[15] For the historical development of classical cost theory, in addition to works already cited, see: Sir William J. Ashley, "An Introduction to Economic History and Theory," *Putnam*, New York, 1894; W. Bagehot, "The Postulates of English Political Economy," *Putnam*, London, 1885; F. Bastiat, "Harmonies of Political Economy," *J. Murray*, London, 1860; James Bonar, "Philosophy and Political Economy in Some of their Historical Relations," *Macmillan*, New York, 1893, and "The Economics of John Stuart Mill," *Journal of Political Economy*, Nov., 1911, pp. 717–725; Goetz Briefs, "Untersuchungen zur Klassischen Nationalökonomie," *G. Fischer*, Jena, 1915; Edwin Cannan, "A Review of Economic Theory," *King*, Longon, 1929; R. T. Ely, "The Past and Present of Political Economy," *The Johns Hopkins University Studies*, vii, Baltimore, 1884; C. Gide, "History of Economic Doctrines," trans. by R. Richards, *Heath*, Boston, 1915; Benedikt E. Güntzberg, "Die Gesellschafts- und Staatslehre der Physiokraten," *Duncker and Humblot*, Leipsic, 1907; L. H. Haney, "History of Economic Thought," *Macmillan*, New York, 1912; Henry Higgs, "The Physiocrats," *Macmillan*, London, 1897; J. K. Ingram, "A History of Political Economy," *Black*, London, 1887; W. C. Mitchell, "Postulates and Preconceptions of Ricardian Economics," in "Essays in Philosophy" (ed. by T. V. Smith and W. K. Wright), *Open Court Publishing Co.*, Chicago, 1929, pp. 39–59; Sir R. H. T. Palgrave, "Dictionary of Political Economy," *Macmillan*, London, 1894; S. N. Patten, "Essays in Economic Theory," *Knopf*, New York, 1923, pp. 19–32, 144–163; Henry Sidgwick, "Scope and Method of Economic Science," *Macmillan*, London, 1885; Leslie Stephen, "The English Utilitarians," 3 vols., *Duckworth*, London, 1900, vols. ii and iii; Georges Weulersse, "Le Mouvement Physiocratique en France (de 1756 à 1770)," 2 vols., *F. Alcan*, Paris, 1910, also "Les Physiocrates," *G. Doin*, Paris, 1931; A. C. Whitaker, "History and Criticism of the Labor Theory of Value in English Political Economy," *Columbia University Press*, New York, 1904.

[16] John Stuart Mill, "Principles of Political Economy," Ashley Edition, *Longmans Green*, New York, 1909, pp. 459–461, 479.

Cf., also, J. S. Mill, "Essays on Some Unsettled Problems of Political Economy," *J. W. Parker*, London, 1844; and Edward S. Mason, "The Doctrine of Comparative Cost," *Quarterly Journal of Economics*, November, 1926, pp. 76–79.

both writers pointed to the importance of neglected scarcity factors (the latter in his doctrine of non-competing groups), thus definitely challenging the underlying free-competition presumptions. At the same time, the utility arguments of the Austrians came to the fore and the gauntlet was thrown down to cost theory as a whole.[17]

With respect to conditions incident to the rise and decline of cost doctrine, three important factors present themselves. First, the early cost theory explained the so-called paradoxes of value better than did the early utility theory; later, the utility theory, transformed by the thorough-going application of marginal refinements, seemed to explain the paradoxes equally well if not better. Second, at the beginning of the 19th century, when classical cost theory attained its most important development, conditions were decidedly in its favor, with free competition being hailed as an economic cure-all; but, by the end of the century, it had become apparent that monopoly was on the increase rather than on the wane, so that cost theory, built on the assumption of an all-pervasive free competition, naturally suffered. Third, the cumulative recognition of the internal shortcomings of cost doctrine completed its downfall. In a series of articles, Fetter gave six causes for this decline, the shift in theoretical discussion from subjective to money costs as a measure of price being regarded by him as the major cause.[18]

[17] N. W. Senior, "Political Economy," 6th ed., *Charles Griffin*, London, 1872. Senior defined cost as "the sum of the labor and abstinence which its production requires; or, to use a more familiar expression, . . . the amount of the wages and profits which must be paid," pp. 98–102. Also, J. E. Cairnes, "Some Leading Principles of Political Economy," *Macmillan*, London, 1874; pp. 60, 72–81; his "The Character and Logical Method of Political Economy," *Macmillan*, London, 1857; and Raymond T. Bye, "The Nature and Fundamental Elements of Costs," *Quarterly Journal of Economics*, November, 1926, p. 32.

[18] Charles W. MacFarlane, "Value and Distribution," *Lippincott*, Philadelphia, 1911, pp. xix-xxi, 19–20, 30–41; also Eugen von Böhm-Bawerk, "The Positive Theory of Capital," trans. by Smart, *Macmillan*, London, 1891, Ch. IV, pp. 146–153. The marginal argument is based upon the alleged operation of the 'law of diminishing utility.' However, with respect to the paradoxes of value, the diminishing-utility reasoning fails to take into account an important distinction between possessive and consumptive interests, mentioned in a quotation from Taussig, *infra*, footnote '40.' *Cf.*, also, footnote '25.'

J. B. Clark and Franklin H. Giddings, "The Modern Distributive Process," *Ginn*, Boston, 1888, p. 20; and Frank A. Fetter, "Price Economics vs. Welfare Economics,"

Much of the later criticism of cost theory came from the utility advocates, who did not, however, realize that some of the objections they voiced might be applied with equal force to their own theory.

Austrian Criticism of Cost Theory

In the historic clash between cost theory and utility theory, such men as Wieser, Böhm-Bawerk, Green, and Patten represented the Austrian contentions. Macvane, among others, fought a losing battle for the cost theory.

Macvane considered the ultimate elements of cost to be "labor and waiting," including both present and past labor and explaining the waiting element in terms of the time needed in roundabout production. Defending his position against the Austrians, he maintained that the latter misconstrued the classical idea of capital as past "labor and waiting" and that these two sacrifices, homogeneously and atomistically construed, determine and measure price.[19]

One objection voiced by the Austrians and most frequently encountered in Macvane's day was based upon the seeming lack of harmony between cost theory and actual market conditions. Wieser and Green pointed out that workers are not as a matter of fact paid according to the effort put forth or the danger involved. Again, Green and others called attention to the fact that scarcity conditions rather than free competition are the rule and that widespread monopoly enables even the so-called marginal producer to secure a surplus over his costs.[20] But these

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American Economic Review, September, 1920, pp. 467–487 (479–480); "Price Economics vs. Welfare Economics: Contemporary Opinion," *American Economic Review*, December, 1920, pp. 719–737; "Value and the Larger Economics," *Journal of Political Economy*, October, 1923, pp. 587–605, December, 1923, pp. 790–803. *Cf.*, also, Fetter's "Definition of Price," *American Economic Review*, December, 1912, pp. 783–813.

[19] S. M. Macvane, "The Austrian Theory of Value," *Annals of the American Academy of Political and Social Science*, November, 1893, pp. 16–18, 28–29, 36; H. R. Seager, "The Impatience Theory of Interest," *American Economic Review*, December, 1912, pp. 834–851.

Macvane, "Marginal Utility and Value," *Quarterly Journal of Economics*, April, 1893, p. 269.

[20] *Cf.*, Frederick von Wieser, "Theory of Value," *Annals of the American Academy of Political and Social Science*, March, 1892, pp. 34–35; David I. Green, "Pain-Cost and

objections refer to market conditions, whereas cost theory has to do with "normal price," regardless of the virtual non-existence of such price. As a result, a wider attack had to be launched by the Austrians.

In criticizing the classical idea of capital as past labor, Wieser objected that no provision was included in cost doctrine for subtracting labor which might today be regarded as wasteful or unnecessary. Macvane attempted to reply, but in doing so, made it even more apparent that his theory presented no logical way of explaining lower costs of production in his day as against those of a century previous. There is no place in cost theory for qualitative improvements in labor and waiting, for the doctrine is wholly quantitative in character.[21]

Similar difficulties regarding qualitative considerations were found in examining the concept of savings, in which consumption factors as a whole are compared with production factors. More than any one else, Patten showed the significance of this comparison.

The physiocrats, Patten recalled, assumed that, apart from agriculture, man consumes all he produces. Under such conditions there is no surplus or savings, and the price of the consumable product may be measured in terms of cost, even in terms of the cost of the wheat necessary to maintain the workers. It was Adam Smith who really destroyed this physical cost measure of price by pointing out that as a matter of fact a surplus is produced in manufactures and commerce as well as in agriculture. Hence the price of the finished product must be greater than its cost. Ricardo, Patten added, thought constantly in terms of a primitive society where there is no surplus or saving, and the whole of classical cost theory, despite Smith's admissions

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Opportunity-Cost," *Quarterly Journal of Economics*, January, 1894, pp. 219–220; J. E. Cairnes, "Political Economy," *Macmillan*, London, 1874, pp. 72–75; Eugen von Böhm-Bawerk, "The Ultimate Standard of Value," *Annals of the American Academy of Political and Social Science*, September, 1894, p. 56; his "Capital and Interest," trans. by William Smart, *Macmillan*, London, 1890; and C. W. Macfarlane, "Value and Distribution," *Lippincott*, Philadelphia, 1899, p. 29.

[21] Wieser, *op. cit.*, p. 30; Macvane, "The Austrian Theory of Value," *Annals*, November, 1893, pp. 19, 26–27.

regarding a surplus, is built on this assumption. In brief, Patten contended: where consumption equals production, a physical cost-of-production measure of price is logical; but where production outstrips consumption, what the consumer can afford to pay (through productive effort plus savings) takes on ever greater importance, especially where monopoly can force most of the productive surplus into the employer's hands.[22]

The Austrians thus presented three fundamental criticisms of cost theory: (1) that no provision is made for eliminating wasteful or needless elements in past labor, as compared with more efficient present labor, and that embodied and commanded labor must therefore be at variance; (2) that qualitative differences in "labor and waiting" are also unprovided for and that these must be assumed to exist to account for decreasing per unit costs; and (3) that since mounting savings prove that production is cumulatively outstripping consumption, ability to pay must be of at least equal importance with cost in price determination.

On one point only did Macvane apparently get the best of the argument with the utility advocates. He exposed the Austrian attempt to reduce subjective cost to money cost, as in his criticism of the "alternative" sophistry.[23] But Macvane and the rest of the cost advocates confused subjective and money costs in an even subtler and thus more pernicious way when viewed in the light of the fallacies already reviewed. The cost theorists thought of subjective cost as normally measuring price wholly in terms of their assumed equivalence between commanded

[22] S. N. Patten, "Dynamic Economics," *University of Pennsylvania*, Philadelphia, 1892, pp. 13–27. Among Patten's numerous writings, see also: "The Premises of Political Economy," *Lippincott*, Philadelphia, 1885; "The Development of English Thought," *Macmillan*, New York, 1899; "The Theory of Prosperity," *Macmillan*, New York, 1902; "Essays in Economic Theory," *Knopf*, New York, 1923.

[23] Mayer, "Pseudo-Scientific Method in Economics," *op. cit.*, pp. 421–425. *Cf.*, also, Macvane, "The Austrian Theory of Value," *op. cit.*, pp. 19–20, 25–26; and "Marginal Utility and Value," *op. cit.*, p. 258. Patten ("Dynamic Economics," *op. cit.*, pp. 27–28) accused Smith and Ricardo of falling into the same error; although his accusation would seem to be too sweeping, it is undoubtedly applicable to those lapses in the Smith-Ricardo reasoning where labor cost is confused with money cost. J. S. Mill also perpetuated this confusion, "Principles of Political Economy," Ashley Edition, *Longmans Green*, London, 1920, p. 583.

money, commanded labor, and embodied labor, and of their unwarranted hypothesis that embodied labor measures both wages and price under "early and rude conditions." If these fallacious assumptions are taken away, every vestige of the classical measure of price through subjective cost would seem to disappear, even under the carefully-guarded though increasingly impotent presumption of so-called normality.

Present Status of Cost Theory and of Objections to It

There would be little if any occasion at the present time to consolidate the arguments for and against classical cost theory, as has been done in preceding pages, if economists by and large appreciated the significance of these arguments. Contemporary lack of such appreciation is evidenced in two directions. On the one hand, a small though growing group (still apparently in the minority) has either become impatient of classical cost dogma and refuses to be concerned with it in any form or has endeavored against odds to clarify various aspects of the pseudo-scientific reasoning. On the other hand, the majority appears to be satisfied with the form and spirit, if not with all the content, of the old dialectic, and continues to perpetuate it (usually with the addition of still more tenuously-drawn refinements), by passing it on to students and to the general public.

In the thought of those who, with further subtleties, continue today to propagate the old confusions, there is presented an informative example of constructive suggestions regarding cost elements united with traditional reasoning of a questionable character, a combination which makes it so difficult to keep pseudo-scientific ghosts laid.[24] With considerable eclectic power,

[24] *Cf.*, Raymond T. Bye, "The Nature and Fundamental Elements of Costs," *Quarterly Journal of Economics*, November, 1926, pp. 30–62; H. G. Brown, "Economic Science and the Common Welfare," *Missouri Book Co.*, Columbia, Mo., 1923, Part II, Ch. II, sec. 3; T. N. Carver, "Principles of National Economy," *Ginn*, Boston, 1921, Ch. 26; Gustav Cassel, "Theory of Social Economy," trans. by Joseph McCabe, *Harcourt Brace*, New York, 1923, Ch. III, Sec. 12; H. D. Henderson, "Supply and Demand," *Harcourt Brace*, New York, 1922, Ch. 10; A. C. Pigou, "Economics of Welfare," 4th ed., *Macmillan*, London, 1932.

Cf., also H. J. Davenport, "The Formula of Sacrifice," *Journal of Political Economy*, September, 1894, pp. 561–573.

the most essential elements in the older classical analysis are picked out, some of the more important difficulties are given emphasis, at a convenient moment the alternative-use maneuver is blandly executed, with resultant muddling of subjective and pecuniary aspects, and, as a modern refinement, the meaning of "real cost" is stretched until it loses its strict classical significance and becomes synonymous with any factor conditioning or limiting supply.[25]

The listing of non-pecuniary supply-limiting factors under a number of significant headings constitutes an important analysis which might profitably be carried further, as might, also, a similar analysis of non-pecuniary demand factors, if the problem of price-determination is to be brought nearer to solution. But the continuance of hoary economic sophistries which attempt, willy nilly, to make "real costs" measure price serves only as a hindrance to constructive thought.

While there is thus a continuance of dialectical and pseudo-scientific reasoning in contemporary cost theory, a much more realistic view of the whole matter is fortunately here and there also in evidence. Analyses are being made of the various forms that subjective costs take and of their effect upon employment distribution in different occupations. At the same time, among those who exemplify this realistic approach, there is an almost complete absence of attempts to reduce different types of sacrifice to measurable or comparable terms, the conviction being held that "the history of English value theory from Ricardo to Marshall" demonstrates that no such common unit can be found.[26]

By and large, the present situation with respect to cost theory is about as follows: On the one hand, it is increasingly taken for granted that "real costs" do not measure price, that they are not homogeneous and commensurable, and that, even if they were, scarcity and monopoly conditions are just as important in price

[25] Bye, *op. cit.*, pp. 30–41, 47, 60–61.

[26] Edward S. Mason, "The Doctrine of Comparative Cost," *Quarterly Journal of Economics*, Nov. 1926, pp. 65–67, 71, 92.

Regarding the shortcomings of classical and neo-classical cost theories from the point of view of modern accounting practice, see: John B. Canning, "Cost of Production and Market Price," *Accounting Review*, September, 1931, pp. 161–164.

determination. On the other hand, with and without an appreciation of the limitations of subjective cost doctrines, further effort is being made to ascertain more precisely the influence of "real costs" upon supply. In this effort, however, confusion between subjective or non-pecuniary costs and money expenses still continues, largely by way of the "alternative" sophistry. . . .[27]

The classical cost argument may now be epitomized as follows: first, it was conjecturally assumed that, in early society, embodied labor controlled exchange ratios; second, under the hypothetical conditions thus assumed, embodied and commanded labor and commanded money would evidently be equal, hence they came to be regarded as equal in modern society; third, scarcity and monopoly were thought of as virtually non-existent, hence it was concluded that market price equals normal price and that these are determined by embodied or commanded labor.

Recapitulating the fallacies and inadequacies in order: (1) A primitive stage of society in which embodied labor measured exchange value or price appears never to have existed. Traditions of forced rather than free exchange, and of predatory power, monopoly, and tribute, seem to have been the starting points of the "values" and "prices" inherited by more civilized descendants, who in slow stages eliminated the worst of the earlier political and social inequities. (2) By means of the "alternative" sophistry and the erroneous assumption of homogeneous units of labor and waiting, the doctrinal superstructure of cost as the measure of price was nevertheless erected. (3) Its crumbling began as scarcity and monopoly conditions came to be recog-

[27] For further data on the present status of cost theory see: Edwin Cannan, "A Review of Economic Theory," *King*, London, 1929, Chs. IV and VII; J. M. Clark, "Studies in the Economics of Overhead Costs," *University of Chicago Press*, Chicago, 1923; H. J. Davenport, "Cost and Its Significance," *American Economic Review*, December, 1911, pp. 724–752; F. Y. Edgeworth, "The Laws of Increasing and Diminishing Returns," in his "Papers Relating to Political Economy," 3 vols., *Macmillan*, London, 1925, vol. I, pp. 61–99; D. H. Robertson, G. F. Shove, and P. Sraffa, "Increasing Returns and the Representative Firm, A Symposium," *Economic Journal*, March, 1930, pp. 79–116; Henry Schultz, "Statistical Laws of Demand and Supply," *University of Chicago Press*, Chicago, 1928, Ch. IV, and "Marginal Productivity and the General Pricing Process," *Journal of Political Economy*, October, 1929, pp. 505–551; Jacob Viner, "Cost," *Encyclopedia of the Social Sciences*, New York, 1931, Vol. IV, pp. 466–475.

nized more and more as the rule rather than the exception, and qualitative differences in labor and waiting had to be admitted to account for decreasing per capita costs from generation to generation. (4) Its downfall was finally completed when a comparison of consumption with production factors brought to light an ever-mounting surplus or savings under modern production and indicated the increasing importance of ability to pay as against cost of production as a determinant of price. The cumulative recognition of these fallacies and deficiencies spelled the doom of the classical cost structure, although the "alternative" sophistry and other phases of the fallacious cost dialectic still continue to persist.

Contemporary Utility Theory

As evidenced in the criticisms of cost theory advanced by the Austrians, there is little question that the Austrian or utility school contributed in important respects to value theory, for at the very least it served to emphasize the significance of considerations which the cost economists had discounted, that is, it placed demand factors on a par with supply factors in price determination.

Despite these contributions, however, the Austrians at no time stopped to analyze the meaning of "utility" as carefully as their opponents had come to analyze "cost," with its "subjective" and "objective" manifestations. That "utility" is to this day a thoroughly vague and ambiguous concept has been pointed out in some detail by the present writer in other connections.[28] Utility theory has nevertheless developed with this vague concept as a base, so that for its understanding one must at the outset accept the ambiguity and within the limitations thus imposed endeavor to evaluate the more immediate claims of its advocates. Since the theory came into prominence in comparatively recent times, we may pass over its beginnings, except as these have already been incidentally touched upon, and take up immediately

[28] Mayer, "The Techniques, Basic Concepts, and Preconceptions of Science and Their Relation to Social Study," *op. cit.*, pp. 475–481.

what it signifies in current thought to its more critical proponents.[29]

To begin with, certain vulnerable contentions of the earlier Austrians have long since been outwardly foresworn, albeit in reality some of them still continue to be held. In recent critical literature, although it is admitted that the psychological basis of utility doctrine may be unsound, its structure over-elaborated, and its earlier contentions regarding utility and demand schedules erroneous, it is nevertheless maintained that its essential arguments remain intact. These arguments are that diminishing utility explains the downward slope of the demand curve and

[29] For the historical background of the utility school, in addition to works by Gide, Haney, and Ingram cited in footnote '15,' *supra*, see: Eugen von Böhm-Bawerk, "Capital and Interest, a Critical History of Economic Theory," trans. by William Smart, *Macmillan*, London, 1890, "The Positive Theory of Capital," trans. by William Smart, *Macmillan*, London, 1891, "Die Österreichische Schule," in "Gesammelte Schriften," *Hölder*, Vienna, 1924, pp. 205–229; Nikolai I. Bukharin, "The Economic Theory of the Leisure Class," tr. from the Russian, *International Publishing Co.*, New York, 1927; H. J. Davenport, "Value and Distribution," *University of Chicago Press*, Chicago, 1908; Oskar Engländer, "Karl Menger's Grundsätze," *Schmoller's Jahrbuch*, Vol. li, 1927, pp. 371–401; Siegmund Feilbogen, "L'École Austrichienne d'Économie Politique," *Journal des Économistes*, 6th ser., number of articles, written in 1911, 1912, and 1913; Frank A. Fetter, "Value and the Larger Economics," *Journal of Political Economy*, Oct. and Dec., 1923, pp. 587–605, 790–803; Jacob H. Hollander (ed.), "Economic Essays Contributed in Honor of John Bates Clark," *Macmillan, New York*, 1927; W. S. Jevons, "The Theory of Political Economy," *Macmillan*, London, 4th ed., 1911; Karl Menger, "Zur Kritik der Politischen Ökonomie," *Hölder*, Vienna, 1887; Wesley C. Mitchell, "Wieser's Theory of Social Economics," *Political Science Quarterly*, March, 1917, pp. 95–118; Simon N. Patten, "The Premises of Political Economy," *Lippincott*, Philadelphia, 1885, and "Essays in Economic Theory," *Knopf*, New York, 1923; Joseph Schumpeter, "Eugen von Böhm-Bawerk," in "Neue Österreichische Biographie, 1815–1918," vol. ii, *Amalthea*, Vienna, 1925, pp. 63–80; William Smart, "An Introduction to the Theory of Value on the Lines of Menger, Wieser, and Böhm-Bawerk," 4th ed., *Macmillan*, London, 1914, and "Second Thoughts of an Economist," *Macmillan*, London, 1916; Thorstein Veblen, "Professor Clark's Economics," and "The Limitations of Marginal Utility," in his "The Place of Science in Modern Civilization," *Huebsch*, New York, 1919, pp. 180–251; Wilhelm Vleugels, "Das Ende der Grenznutzentheorie?," *C. E. Poeschel*, Stuttgart, 1925; Otto Weinberger, "Die Grenznutzenschule," *Meyer*, Halberstadt, 1926; Friedrich von Wieser, "The Austrian School and the Theory of Value," *Economic Journal*, March, 1891, pp. 108–121, "Natural Value," tr. by C. A. Malloch, *Macmillan*, London, 1893, "Über den Ursprung und Hauptgesetze des Wirtschaftlichen Werthes," *Hölder*, Vienna, 1884; Allyn A. Young, "Jevon's Theory of Political Economy," in his "Economic Problems, New and Old," *Houghton Mifflin*, Boston, 1927, pp. 213–231.

that the resultant margin determines price.[30] For closer inspection, these arguments will be divided into four underlying presumptions: first, the hypothesis of universally-operating diminishing utility; second, the alleged importance of the margin; third, the assumed relation between utility curves and the negatively sloping demand curves; and, fourth, the resultant putative effect of such assumptions upon price determination. The first three of these presumptions will be discussed in the remainder of this section; the fourth presumption, in the next section.

First: Diminishing utility was once thought of as all pervasive, but since the beginning of the present century a number of exceptions have been noted, some of them relatively unimportant but others of disconcerting significance, as, for example, in the case of the collector of coins or the matcher of pearls, who does not desire each succeeding coin or pearl the less, but the more.[31] An ingenious attempt is sometimes made to dispose of such exceptions by choosing a "proper set" as the unit, but this merely emphasizes the fact that within these "sets" diminishing utility is inapplicable.[32] And when it comes to the desires of the mind and spirit, it is obvious that there is quite a range of human experience in which increasing rather than diminishing utility operates, at least for a time.

If, in addition to these considerations, rate of consumption is,

[30] *Cf.*, writings of the Austrians referred to in footnotes '20' and '22', *supra*; also Jacob Viner, "The Utility Concept in Value Theory and Its Critics," *Journal of Political Economy*, August, 1925, pp. 369–387. Viner's references to Marshall, Wicksteed, and Walras in these pages add nothing with respect to the questions at issue.

Cf., also, F. S. Deibler, "Principles of Economics," *McGraw-Hill*, New York, 1929, pp. 188–189; and Frank Knight, "Marginal Utility Economics," *Encyclopedia of the Social Sciences*, New York, 1931, Vol. V, pp. 357–363.

[31] Viner, *op. cit.*, pp. 378–380. It is well to point out here that in this present analysis no attempt is made to differentiate between diminishing 'utility' and diminishing 'desire,' since the utility advocates use these concepts more or less synonymously for the purposes of the present argument, 'desire' being used in a very general way to cover 'interest,' 'want,' 'need,' etc. *Cf.*, R. B. Perry, "General Theory of Value," *Longmans Green*, New York, 1926, pp. 550–551, and his "Economic Value and Moral Value," *Quarterly Journal of Economics*, May, 1916, pp. 447–449. What Perry says about value applies with equal force to utility. *Cf.*, also, H. J. Davenport, "The Economics of Enterprise," *Macmillan*, New York, 1913, p. 86. He says, "Utility is the mere fact that a thing is desired."

[32] Viner, *op. cit.*, p. 378.

as it should be, brought into the picture, there is in the experience of all of us a still wider area in which neither diminishing nor increasing but constant utility is the rule.[33] So that, taking it all in all, the utility or desire curve is not uniformly negative in slope. It may just as often be positive. And for the most part it is apparently zero in slope. What, then, becomes of the so-called law of invariably diminishing utility or desire? For one thing, it is certainly far from being a universal phenomenon in economic activity.

Second: The marginal concept is usually tied up with diminishing utility, the assumption being that the last increment of utility is always the smallest.[34] But we have just seen that the final unit may be the largest (under increasing utility) or the same as those that precede (under constant utility). In addition, Marshall's suggestion pertaining to the use of the whole utility schedule (rather than the final increment only) and the fact that a given stock of goods is not always increasing (as usually assumed) would seem to make the modifier "average" more reliable than "marginal."[35] A most incisive criticism of the marginal concept as such was given by Carlile a quarter of a century ago, in which it was labeled misleading, nonsensical, and wholly retrospective, providing no ground for prediction, whether with respect to price or otherwise. On the whole, the margin is generally recognized today as of no greater significance than other portions of demand and supply in price determination.[36]

[33] *Cf.*, H. E. Miller, "Utility Curves, Total Utility, and Consumer's Surplus," *Quarterly Journal of Economics*, February, 1927, pp. 292–316.

Cf., also, L. M. Keasbey's analysis of "Prestige Value," *Quarterly Journal of Economics*, May, 1903, pp. 456–475, especially p. 472.

[34] *Cf.*, H. J. Davenport, "The Economics of Enterprise," *op. cit.*, p. 90.

[35] *Cf.*, Alfred Marshall, "Principles of Economics," *Macmillan*, London, 4th ed., 1898, Vol. I, p. 592; also Viner, *op. cit.*, pp. 371, 385; and Davenport, "Economics of Enterprise," *op. cit.*, p. 94.

[36] W. W. Carlile, "The Language of Economics," *Journal of Political Economy*, July, 1909, pp. 434–447; and his "Economic Method and Economic Fallacies," *Arnold*, London, 1904.

Cf., also, Davenport, "Economics of Enterprise," *op. cit.*, p. 94, and his frequently repeated statement that it is *at* the margin, not *by* the margin, that price is fixed.

Third: Since utility curves do not appear as a rule to be negative in slope, demand curves (almost always negative) can hardly be derived from them in the uniforn and inevitable way insisted upon by utility theorists.[37]

Market demand curves and individual demand curves are often confused and should therefore be examined separately.[38] Assuming a hypothetical situation in which the utility of a given commodity (say oranges), the number of units demanded, and the rate of consumption per person are all constant, the utility curves for different individuals would by assumption be horizontal and coincident. But would the resultant market demand curve for oranges also be horizontal? It certainly would not, in so far as there remained differences in purchasing power making for differences in willing-to-pay prices. The market demand curve would here be negative in slope due to inequalities in income. No supposed universal law of diminishing utility is necessary for an explanation of the negative slope.

Furthermore, at least part of the foregoing hypothetical situation is always contrary to fact. "Men differ in desires and in the degree and manner in which things appeal or appear to offer service."[39] Hence differing tastes or desires for the same commodity among individuals provide another explanation, in addition to inequality of incomes, for the negative slope of the market demand curve, again quite apart from diminishing utility considerations.

Turning to individual demand, a confusion between acquisitive and consumptive interests is first to be noted. In acquisition, pertaining to demand, the economic importance of each unit is the same, even though, in consumption, the utilities involved may be of a diminishing, increasing, or constant character.[40]

[37] Viner, *op. cit.*, p. 378, 381; also, his "Some Problems in Logical Method in Political Economy," *Journal of Political Economy*, March, 1917, pp. 236–260.

[38] F. R. Fairchild, E. S. Furniss, and N. S. Buck, "Elementary Economics," *Macmillan*, New York, 1926, Vol. I, pp. 255–257.

[39] Davenport, "Economics of Enterprise," *op. cit.*, p. 97.

[40] *Cf.*, Fairchild, Furniss, and Buck, *op. cit.*, Vol. I, pp. 300–304; also Davenport, "Economics of Enterprise," *op. cit.*, pp. 88–91.

F. W. Taussig, "Principles of Economics," 3rd ed., *Macmillan*, New York, 1921, Vol. I, pp. 121–122. Taussig's implication, in these pages, that economic importance comes 'to

Hence if an individual's purchases (as we are continuing to assume) were always restricted to a set number of units, his individual demand curve would be horizontal. But in actual life he is not thus restricted; he may buy more when the price is down, less when it is up. His individual demand curve is therefore as a rule negative in slope, and this situation maintains even though the utilities derived are fairly invariable (if we continue to assume a constant rate of consumption). It is the universal desire to make the dollar go as far as possible which explains the negatively sloping individual demand curve, quite apart, once more, from any alleged diminishing utility.

In brief, unequal incomes and desires and prudential buying in the light of fairly fixed budgetary allotments of expenditure are competent and sufficient to explain the character of demand curves without recourse to utility considerations, thus vitiating one of the main contentions of the utility advocates.

Utility and Price Determination

The other major contention of the utility school, that marginal utility determines price, has already been severely challenged in the foregoing analysis. Whatever force it was supposed to have gathered from the universally-diminishing-utility, demand-concomitant-with-utility, and marginal assumptions would seem for the most part to have been dissipated. But there is still a hard core of argument left which is not inherently related to any of these presuppositions. Stripped of other considerations, the remaining contention appears as an attempt to measure desire or utility in terms of price offers, and the argument narrows down to a variant of the alternative-use doctrine.

It is now generally conceded, even by utility proponents, that inequalities of income and variations in personality make it impossible to compare willing-to-pay prices and utilities as between individuals. Such an attempt is a "pernicious error . . . the result of slipshod thinking."[41] It is thus within the indi-

the same thing' as marginal utility is not agreed to by the present writer. *Cf.*, also, Perry, "Economic Value and Moral Value," *op. cit.*, pp. 451–453.

[41] H. J. Davenport, "The Economics of Enterprise," *Macmillan*, New York, 1913, pp. 92, 97. Jacob Viner, "The Utility Concept in Value Theory and its Critics," *Journal of Political Economy*, August, 1925, p. 370.

vidual economy that the utility advocates focus their final arguments, although, even here, they construe the problem not absolutely but relatively.

The method employed, that is, measurement in terms of proportionality, is well illustrated in an analysis by Davenport, who concluded with his well-known "ratio of equality" between price offers and utility, by which he apparently meant that there are always present in each of us two competing and alternative desires and that between them there exists a proportionality with price offers.[42]

Let us for a moment examine these implications. Is it a fact that two competing desires are always present in each individual when a price is offered? Psychological research has apparently indicated no such situation, but if as a "schematic" assumption it is supposed to help in price theory, we may accept it tentatively and note where it leads.[43] Following the argument on its own ground, we observe immediately that not only is a duality of desires here assumed but the desires are at the same time assumed to be equal in intensity, that is, x:y::5:5. A "ratio of equality" naturally follows from such presumptions, but no proof is anywhere offered as to their truth.[44]

A concrete example or two will serve to demonstrate the wholly hypothetical character of these conjectures. If Mr. A. at a given time is willing to pay $4.00 for a hat and the same sum for a pair of slippers, it is here supposed that the intensity of his desire for each is equal. If at the same time he is willing to

[42] Davenport, *op. cit.*, pp. 84, 93, 104. Viner apparently has the same idea in mind, though he expresses it less clearly, when he says that price-offer is the result of 'contact' of two alternative desires in the individual.

[43] *Cf.*, Davenport, *op. cit.*, pp. 100–101.

It is of course true that if we buy one thing we cannot with the same money buy something else, just as it is true that if we walk up one street we cannot at the same time travel on another. But that we cannot buy something does not imply necessarily that we desire it, any more than not being able to walk on another street implies necessarily that we desire to do so.

[44] Of course, if the actual and the assumed desire were in fact precisely equal, no choice between them could ever be made and thus no purchase would result. As in the Greek legend, our desires would remain in a condition of eternal suspense. *Cf.*, Davenport, *op. cit.*, p. 101, Note.

pay $8.00 for a pair of shoes and $40.00 for a suit of clothes, it must then be supposed, presumably, that he wishes to cover his body ten times as intensely and his feet twice as much as his head. To suppose any comparisons of such a character is obviously absurd.[45]

It is of course true that a desire varies in intensity and that the intensities may be roughly compared, but this pertains to comparisons within a given desire, not between desires, so that, without empirical evidence, there is no basis for the proportionality argument. If the attempt to measure desires between individuals is a "pernicious error," the same indictment must apparently be entered with respect to efforts to compare desires within individuals. In fact, a better case might be made out for the comparison of an orange want between two like individuals than for comparing an orange want and a swim desire in the same person.[46]

The alleged proportionality, therefore, between desires and price offers is out of harmony with reality: first, because desires as such are more or less unlike and unique; second, because the assumed "ratio of equality" is merely an imaginative conjecture; and, third, because the assumed alternative shadow desire, always equal to and competing with every actual desire, is contrary to the facts of experience.[47]

It thus seems clear that the proportionality argument, while an excellent piece of dialectic, makes use of presumptions at variance with psychological facts. There are several other problems, however, having to do with the alleged relation between

[45] *Cf.*, Joseph Mayer, "Comparative Value and Human Behavior," *The Philosophical Review*, forthcoming issue, 1936.

[46] *Cf.*, W. C. Mitchell, "Bentham's Felicific Calculus," *Political Science Quarterly*, June, 1918, pp. 161–183; John Laird, "The Idea of Value," *University Press*, Cambridge, England, 1929, pp. 25, 340–347, 359.

Davenport, *op. cit.*, p. 97; and Mayer, "Comparative Value and Human Behavior, *op. cit.*

[47] *Cf.*, also, J. A. Hobson, "Work and Wealth," *Macmillan*, London, 1914, p. 334: "Quantitative analysis, inherently incapable of comprehending qualitative unity or qualitative differences, can only pretend to reduce the latter to quantitative differences. What it actually does is to ignore alike the unity of the whole and the qualitativeness of the parts." The whole of Hobson's last chapter is apropos in this connection.

utility or desire and price offers which should be further explored before our examination of the position of the utility school can be concluded. One is the important matter of pecuniary choice. If the satisfaction of one basic desire cannot be substituted for the satisfaction of another—if wants are not for the most part interchangeable and commensurable—how explain the fact that apparent conflicts of desire do occur and that these appear to be the rule in daily experience rather than the exception?[48] Part of the answer here is that many of the seeming conflicts are evidently not between fundamental desires as such but lie in a choice between different commodities, each of which will presumably satisfy a given desire to some extent; or the conflict may be but temporary, resulting merely in a postponement of the fulfillment of one of the desires. These common situations are clearly indicated in everyday experience. For the most part it seems that there is no extensive fundamental, irreconcilable conflict at all.[49]

One of the important considerations in the matter of seeming or actual pecuniary conflict is apparently whether the price encountered is regarded as "fair" or "right." We tend to accept customary price without question; nor is there any rationality in attempting to equate habitual price with amount of utility, since we more or less mechanically ratify whatever price situation we are born into. We take for granted what we think we have to pay, and as a rule raise the question of "fairness" only when prices go up. To assume that habitual price, to which we are introduced in childhood, has by some magic later come to measure utility or desire, is evidently making the wish father to the thought. If the price has risen, it may be justified as "fair" on grounds of rising costs, but the basis of acceptance still apparently remains the "customary price," not cost of production or amount of utility.[50]

[48] *Cf.*, Davenport, *op. cit.*, p. 100.

[49] *Cf.*, H. W. Stuart, "Phases of the Economc Interest," in "Creative Intelligence," ed. by John Dewey, *Henry Holt*, New York, 1917, pp. 310–340; also, Mayer, on Preference, in "Comp. Value and Human Behavior," *op. cit.*, especially distinction between "more than" and "rather than" comparisons.

[50] *Cf.*, H. W. Stuart, *op. cit.*, pp. 318–322; Mayer, "Comparative Value and Human Behavior," *op. cit.*, especially Hobson's illustration of an artist's painting; E. H. Downey,

Another question that needs to be considered here centers upon the useful distinction in economics between free goods and economic goods, in which connection it is sometimes overlooked that free goods (like air and water) have utility and that this is not measured by price. Nor is there any reason to believe, prestige values apart, that the utility of a free good, like air, necessarily changes when it becomes economic and commands a price. If nature were to supply all goods gratuitously, as is the situation with the lilies of the field, goods would still possess varying types and degrees of utility for man, since he would continue to have wants and desires to be satisfied. Hence there is no necessary relationship, from this point of view, between utility or desire and price.[51]

Applying the foregoing considerations to economic goods as such, we observe that nature is bountiful in the one case as in the other. There is a varying amount of "free gift" in all economic goods, man merely adding his energy to nature's in achieving a joint result. Nor is man's contribution always in the same ratio with nature's. In the "making" of strawberries or the ocean, man adds little; in the "making" of a house or an automobile, much more. It is possible to calculate the money cost of what man contributes, but the utility of the resultant good comprehends both nature's share and man's share. Bringing the strawberries to the table (man's contribution) and then omitting the strawberries (nature's contribution) is meaningless from the point of view of want satisfaction. Furthermore, the total utility is indivisible.

Does willing-to-pay price measure this indivisible total utility?

"The Futility of Marginal Utility," *The Journal of Political Economy*, April, 1910, pp. 253–268; and C. H. Cooley, "The Institutional Character of Pecuniary Valuation," *American Journal of Sociology*, January, 1913, pp. 543–555. Cooley says: "The influence of the market is not secondary either in time or importance to that of the person; it is a continuous institution in which the individual lives and which is ever forming his ideas. . . . The institution largely dictates the valuation which it afterwards equilibrates. . . . In pecuniary matters one accepts in a general way the current values" (p. 547).

[51] In the light of the distinction already made between possessive and consumptive interests, it is unnecessary here to comment upon the specious reasoning which leads to the absurd conclusion that the utility of a free good is zero. *Cf.*, also, L. M. Keasbey, "Prestige Value," *Quarterly Journal of Economics*, May, 1903, p. 461.

The stock illustration of a man about to be deprived of air or water provides no criterion for ordinary purposes, for it merely indicates that there are no alternatives to air and water. What we are ordinarily willing to pay rather than go without is doubtless determined in part by the price of the nearest substitute, in part by our relative purchasing power, and in part by what we are accustomed to pay for the good in question; but there is apparently no rationality in assuming that as a rule we are willing to pay a price for something that it is possible to secure gratuitously. Since nature's contribution to utility cannot be calculated, whether in terms of price or otherwise, it is futile to attempt to measure in money terms the satisfaction an economic good affords to man.

There is still a final question in this connection. Are we not willing to pay more to satisfy a relatively intense want, and does not this fact provide a basis for the measurement of utility? We would of course be willing to pay more to satisfy an intense want if necessary, but since nature may contribute much or little to the satisfaction of such a want and since there is no way of ascertaining the proportion, we are in no position to say that a price offer, whether large or small, is related to the intensity of the desire or to the amount of the utility, which is of course to assume for the moment that the intensities of different wants or desires can be compared—a very dubious assumption.[51a]

The foregoing analysis of utility theory may now be epitomized: (*a*) diminishing utility is not a universal phenomenon, since increasing utility is probably just as prevalent, and a uniform utility is undoubtedly even more prevalent; (*b*) as Marshall indicated, the alleged determinative significance of the margin in utility analysis is without foundation; (*c*) the negative slope of the demand curve is better accounted for by unequal incomes, differing desires, and prudential expenditure, eliminating the need for the fiction of a universally-diminishing-utility; (*d*) utility thus does not determine market price,—first, because the universally-diminishing-utility and marginal assumptions are largely inap-

[51a] *Cf.*, Mayer, "Comparative Value and Human Behavior," *op. cit.*

plicable, second, because the special ratio-of-equality argument is contrary to fact, and third, considering analyses based upon customary price, pecuniary choice, and the bounties of nature in supplying utilities to free and economic goods alike, because there is no correlation between desire or utility and price offers. Thus both major contentions of the utility school would seem to be clearly fallacious, while other more realistic hypotheses are available to explain the relation, or lack of relation, between so-called utility and price.

Combining utility and cost theory criticisms, there seems little in this classical and neo-classical structure of dialectic which is not pseudo-scientific, fallacious, or wholly inadequate to explain modern economic phenomena.

(To be continued)

REVIEWS AND NOTES

THE UNIVERSE OF OPERATIONS

The Nature of Physical Theory. By P. W. Bridgman. Princeton University Press, 1936. 138 pp. $2.00.

This small book, an expansion of three Vanuxem lectures given at Princeton University in December 1935, is packed solidly with characteristic Bridgman hardheaded opinion, covering experience, thought, language, logic, mathematics, relativity, and wave mechanics. The brilliant exponent of operationalism (the demand that physical concepts be defined in terms of physical operations) writes himself down more clearly than he ever did in his "Logic of Modern Physics." The reasons for this are that modern physics has had a few years more in which to score both failures and successes, and that the critic has had a chance to gain courage in combat. Besides, it is very wise to wait till the first wild extravaganzas are over.

The volume is modestly introduced, yet it has a dogmatic ring. Such an overtone appears when one is pushing any kind of a "mouse-trap" or Pyrrhonistic point of view. The sceptic who tacks on a "why" to everything, the philosopher who denies reality to anything but the absolute, the extreme positivist who in the first flush of reform dismissed many a useful notion as "nonsense," the bland solipsist, have an irrefutable manner about them, because once you have bitten of their cheese their mouse trap is inevitable. Thus, the skillful Freudian can take, let us say, an opponent of the idea of wish fulfillment and "explain" his opposition as a wish fulfillment. You are damned if you do and you are damned if you don't. The only way I have found of evading such traps is to risk an agreement in order to turn the tables. Accuse the Freudian of wish fulfillment in believing in his theory of wish fulfillment. He usually does not like that at all, and it turns out that he really believes that the wish is father to the thought, except in the case of his thought. This is as pathetic as the belief: All men are mortal except myself. Bridgman seems to be pressing operationalism to such a point. He can probably ruin almost any position by persistent pressure for an

operational definition of meaning, but apparently thinks he is immune himself when he resorts to "ultimates." In his philosophy such a position is occupied by some of the old standby's as "brute fact," "activity," and "recognition." Although he recognizes science to be circular he avoids closing the circle in his own treatment. This, I think, is a gigantic evasion of the whole issue. His demand for operational criteria has no operational basis, and rests like the world-turtle of old cosmologies on a bottomless ocean of infinite regression. It is terribly unoperational to say (p. 13) "In the last analysis science is only my private science." What can an operationalist mean by a "last analysis?" He definitely takes sides on a pseudo-problem, because he cannot eradicate in himself the very type of idealism he decries in Eddington and Jeans. His assertions as to the subjective character of science have no objective status at all on his own criteria. His subsequent treatment of the chain: experience, thought, language, has what appears to me to be a great disadvantage, and to him an advantage, of being rooted in a kind of mysticism of the internal with its introspective privacy and intuitional insight. For example, it is intuitive introspection that reveals to him the *incompleteness* of the account of thought, but by his own dicta it is meaningless to speak of completeness or incompleteness till the whole show is over. A man who most seriously states, as he does, that before Π was proved to be a transcendental number in 1882 it was meaningless to ask whether it was or was not one in 1881, must not himself ask questions where he cannot give an operational criterion for a decision. I should not expect him to announce a decision one way or the other when he cannot give directions for making a decision.

Let us hear what Bridgman has to say about the Law of the Excluded Middle:

> "A thing is either A or it is not—A. Operationally, if this statement is to have rea content and not be merely a concealed tautology, it must imply something like the following: I am given a certain object to examine; if it has the property A then I shall find that certain operations performed on it give certain results; similarly if it has the property non-A then I shall find that certain other operations performed on it give certain other results. The law of the excluded middle makes the statement that if I operate according to the one or the other of these two rules of operation, then I shall find that one or the other of the operations has yielded a positive result. Obviously as a general statement about any two sets of operations this is going much too far. . . ."

Suspicious as he is, does Bridgman himself avoid the invocation of the Law? He says that the first test had to be constructed so that it either

gives a positive or negative result (invocation of the law) but he himself must make sense of his denial that one of the operations leads to a positive result. I imagine he thinks in two alternatives. Under the word "Obviously" there is a mental process which also suspiciously smells of the law of the excluded middle. Bridgman further claims that the law does not hold for the alternatives true and not-true for statements. Again, his own statement seems to beg for exemption. Furthermore, it is a generalization, "a correct description of *any* case," which he shows adequately on his assumptions cannot be subsumed under the law if the class is open or infinite. His generalization about the truth character of sentences deals with an open class and has the difficulty brought up for the difference between Π of 1881 and of 1882. Not all sentences have been examined by 1936 or can be declared on non-empirical grounds to be examinable or not. That is taking up Bridgman's contempt for "mere tautologies" seriously and strictly.

Supplementing Bridgman's trend to solipsism is a plaint, reminiscent of Bergson, that the unfortunate law is "merely an aspect of the trait of all thought of endeavoring to analyze experience into little frozen bits, and neglects the consideration that experience is essentially activity." The comparison to Bergson may not flatter, but nowhere does Bridgman give anything but a metaphysical notion of activity or of the mystic word "essentially." This is precisely what operationalism cannot reach, any more than any other "ism." All he has demonstrated is that analysis is something different from living through a process. Surely analysis has a different goal and seeks a little of what is common in all situations rather than everything that is true about one situation. Why should that be a reproach to analysis?

Bridgman's further discussion is very interesting. In the course of his treatment of mathematics, which he finds empirically rooted, he suggests a possible break-down of the concept of number for atomic phenomena and points out the remoteness of the mathematical concept of time from the time of experience. This seems to him dreadful because he stresses over again the necessity of reliving phenomena by science, an objection, which, I admit, would be strong against art, but which is not cogent to science at least in its present development. One is grateful, however, for Bridgman's rugged persistence in stressing the obvious but sadly neglected fact that a mathematical or physical formula is formula *plus text*. The symbols, their use, their scope, the setting of boundary conditions, are all described by the text and not by the formula. A very severe critique of the relativity theory (especially the

general theory) is based on a discussion of the suppressed and unanalyzed texts. He clamors for closer specification of such terms as "rigid," "clock," beyond statements "more or less intuitive in character" (the enemy must defend his unanalyzables!). A good case is made out against a confusing notion of "events" and against the characterization of Nature as consisting of pointer readings. World lines are world lines of something. Covariance is not a necessity of relativity. How, asks our critic, can one tell that unanalyzables mean the same thing to two observers when they may perceive the "same" thing by different senses, one yellow light by the eye, the other infra-red by the finger? The problem of discreteness becomes very serious. Bridgman seems to be annoyed that in a theory of *relativity* "it appears to be necessary to assume an absolute significance of language and meaning." In the same spirit he proceeds to question the meaning of the term "law of nature" and balks at extrapolations to the entire universe. In other words he unburdens on relativity many objections that apply to any physical theory. Hence there is a little temptation to me again to cry "tu quoque" when Bridgman forgets himself here and there to speak of "laws" on a "cosmic scale" which he should like to see developed.

Wave mechanics is complimented on somehow approaching the qualitative character of experience more closely than any other theory, because of its treatment of error. He follows the fashion (strange for Bridgman!) of thinking that the uncertainty relation prohibiting an indefinite refining of the precision of measuring has changed the situation with regard to the discovery of a unique law of nature. I wish to point out that even in the classical picture no one ever hoped for an absolutely accurate measurement or could show that refining of measurement could lead to a real limit. Theoretically myriads of laws were possible on the classical procedure, if we but magnified the scale on which we plotted our data. Similarly, Bridgman's claims that quantum theory cannot be essentially complete, is not distinctive for quantum theory alone. I should expect an incompleteness in all theory from a larger point of view, and a larger point of view is always possible.

The concept of probability (on an objective basis) is given short shrift by Bridgman as operationally such as to leave us quite uncertain whether it applies *at all* (p. 102) in any given situation, or whether it did apply in the past. This uncomfortable situation eventuates in wave mechanics being one unavoidably of the mathematical model (p. 115). The physical situation is not reproduced and God is *not* a mathematician. Bridgman pleads for a natural intuitive basis (not a

classical one in terms of unanalyzable particles) for wave mechanics. This leads me to ask what Bridgman of 1946 might be saying if he succeeds in following his hopes which gleam thinly through his pessimism. Something makes me think that he might turn dialectical materialist. Attest to this: (p. 123) "*Chance* has no meaning except in a setting of order. (P. 128) I very much question whether one can even think of physical waves without assuming in his thought that the medium which carries the waves is itself composed of particles." His present theory, on account of his constantly setting of the trap of operational definition, is characterized by his own very clever quip on probability (p. 102): "What theory is it that is not correct unless it leaves open the possibility that it may be incorrect? Answer: the theory of probability." I should say, change the answer to: the theory of operationalism.

WILLIAM MARIAS MALISOFF.

HUMANIZING SCIENCE

Philosophy and the Concepts of Modern Science. By Oliver L. Reiser. New York: The Macmillan Company, 1935, pp. xvii–315.

Heinrich Heine in his *Harzreise* was visited one night by the ghost of the learned Dr. Saul Ascher. Dr. Ascher urged Heine not to be afraid that he was a ghost and proceeded to give a logical demonstration, based on Kant, that there could be no ghosts. Unfortunately as he was leaving, Dr. Ascher reached into his waistcoat pocket and produced, instead of his watch, a handful of worms. Dr. Ascher, with his abstract legs, believed that "Die Vernunft ist das höchste Prinzip!"

If Heine were living today Dr. Ascher's slogan would have been; "Die Wissenschaft ist das höchste Prinzip." Dr. Reiser, a philosopher who has made it his business to have a wide knowledge of science, is not at all sure whether he accepts this premise. There are quite a number of unscientific ghosts that visit a scientific man after a too-hearty supper but which dare not show their faces in his daylit laboratory. Dr. Reiser feels confident that these are real, and that physical science gives "a poverty-stricken picture of reality." He feels that the mechanistic credo of science can no longer confront the poet and the idealist with an open smile of condescension. His early chapters depict the crisis in both scientific and lay thought arising from the waning confidence of the scientific man in some of his own tenets and from the growing lay conviction that science does not, after all, give us so much insight into nature or control of our future.

Dr. Reiser is nevertheless inclined to think that there may be some-

thing to this business of "science." With the help of the gestalt-concept, emergence and what he calls "naturalism"—the idea that, even for science, "anything goes"—he achieves the task Dr. Ascher said was impossible: reconstructs, from what he believes are scientifically sound premises, the ghosts of the poet and the idealist, God and the religious attitude to the world.

Dr. Reiser begins by affirming his faith that a phenomenological, antimechanistic approach to nature is quite as valid for science as quantitative materialism. Yet as he goes on, he keeps forgetting. He sees in the phenomena of visual perception "the link between logic and physics" that is to enable him (with a clear scientific time-sheet) to soar into the whither-never of the empyrean. In this he still talks of the atomistic findings of psychophysics as if they were the primary reality, of higher authority than the experience of St. Paul or Thomas Paine (or even the unknown soldier) who knew nothing of *Psychological Abstracts*. When he comes to his part II ("Philosophy and the Social Sciences") again Reiser's main concern is to show that historical, ethical or religious experience can be rendered respectable in the eyes of the mechanist. Dr. Reiser even tries some evangelical work among scoffing behaviorists by depicting religious aspiration as a sort of glorified heliotropism. Did he remember that Baal was a sun God? . . . On the other hand, it is natural that a Pittsburger should think of the sun with feelings of religious awe. His error here is that he tries to find religious aspiration in the most irreligious of the psychologies.

In Part I ("Philosophy and the Physical Sciences") when he is giving an account of some of his earlier published papers, Dr. Reiser is often very penetrating and clearheaded. Mathematicians, however, will upset by a recurrent vagueness. Chemists will find one or two misconceptions in Chapter VII; but these things are to be understood in a work which embraces every field of science.

However, as regards the general direction in which his thought is moving, Dr. Reiser deserves the warmest praise. That the philistenism of a narrowly-conceived "science" should make professional men close their eyes to aesthetic and spiritual realities is the major metaphysical psychosis of our time. Reiser's heart is in the right place: i.e. in the clouds.

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A TRIBUTE TO WHITEHEAD

Philosophical Essays for Alfred North Whitehead. Longmans, Green & Co., New York, 1936. Pp. vii + 248. $3.00.

This volume consists of nine essays written by teachers of philosophy who were formerly students of Professor Whitehead, and published by them on the occasion of his seventy-fifth birthday. The essays deal with a variety of independent subjects. Seven of them are relevant to the philosophy of science. F. S. C. Northrop's, on "The Mathematical Background and Content of Greek Philosophy," is unfortunately dependent upon failure to distinguish between the logical relations among mathematical ideas developed by various Greeks, and the temporal order in which these ideas appeared and in which they passed from one philosopher to another. Seemingly profound insights, which at one stroke illuminate past doctrines and solve present problems, are then easily obtained. Whitehead is probably one of the very few men living at any time, whose sense of history and acute analytic ability have enabled him to make compound illuminations of this sort time and again, without fallacy. The prophecy may be hazarded that the particular fallacy Northrop committed will often distinguish the writings of Americans who admire Whitehead.

Northrop's reconstitution of Greek doctrines has another aspect. He regards it as prolegomenon to solution of the problems bequeathed to mathematical philosophy by Cantor's foundational work. For the contradictions latent in this work "are so fundamental that they appear in the logic as well as the technical mathematics. The significance of this for contemporary logic, mathematical science and philosophy should be obvious: Traditional modern theories of these subjects cannot be trusted. New mathematical, logical and philosophical theory must be constructed." But shall we begin the reconstruction by trusting Northrop's conjectural interpretations of ancient theories of these subjects? Of course it is often valuable to examine the teaching of history relative to a given problem; and possibly Northrop has got hold of some valuable facts—but they will have to be differently demonstrated. Meanwhile, it is possible that the Cantorian contradictions have been eliminated in the paper, "A Theory of Classes Presupposing No Canons of Type," just published in the *Proceedings of the National Academy of Sciences* (XXII, No. 5) by Willard V. Quine, the author of *A System of Logistic* (Harvard University Press, 1934).

Dr. Quine contributes to the present volume an essay called "Truth by Convention," which is indispensable to those interested in this topic.

It is a careful, technical search for the precise sense in which mathematics and logic can be said to be "true by convention." The author extends as far as possible the use of definitional abbreviation as a mode of convention; and he similarly treats the process of circumscribing the meaning of primitive notions (mathematical, logical, and empirical) through conventional assignment of truth wholesale to various of their contexts. He concludes that the results do not provide any clear and true meaning for the assertion that mathematics and logic are distinguished from the other sciences by proceeding wholly from linguistic convention. He finds one interesting interpretation: it may be considered "philosophically important" to apply the technique of conventional truth-assignment to logic and mathematics in order to "forestall awkward metaphysical questions as to our *a priori* insight into necessary truths,"—whereas there is no need to make this application to the empirical realm, where it is possible but even more elaborate. On grounds of logic, Dr. Quine declines to indorse this suggestion. It will seem to many readers that its character affords an immediate ground for refusal: a technique for the systematic production of fiat is to be used as a defensive instrument to ward off metaphysicians. The idea smacks of international politics: let us present our opponents with a *fait accompli*. But if there is no serious metaphysical question concerning logical and mathematical truths, none need be forestalled. If there is a real metaphysical question, it cannot be forestalled by fiat. The conventionalist view here is one that could possibly find general acceptance for a time; but it would be a stop-gap, not a solution for problems.

Henry S. Leonard's essay, "Logical Positivism and Speculative Philosophy," is completely misnamed, since it is about neither of these, but is instead a remarkably fresh treatment of that hackneyed theme, "scientific method." Evidently the essay was intended to be longer, or to have a sequel which would establish Leonard's novel ideas of the relations between philosophic speculation, logical positivism, and epistemology. Here he goes only so far as to sketch his concept of the "social given," which is intended to provide criteria for distinguishing between worthwhile and foolish speculation.

For the last decade or two, philosophers of science have been acutely conscious of the situation arising from the demonstration that the certitude of mathematics results from its independence of any necessary connection with empirical fact. The situation was painfully sharp-cut: the presentations of sense-experience on the one hand, abstract systems on the other. "At least it appears that we must accept a kind of

double-truth," wrote C. I. Lewis in his Preface to *Mind and the World-Order*. Lewis's book was the first great unifying treatment of this situation by an American philosopher steeped in symbolic logic. The essays by Quine and Leonard in the present volume evidence the working of the tradition which Lewis crystallized, of careful study of both poles of the situation without recourse to metaphysics. An important possible contribution toward unification occurs when Quine, in the course of his paper, gives reasons for rejecting the interpretation of mathematical truths (now prevalent among laymen and mathematicians alike as a result of the repeated quotation of famous statements from Russell's *Principles of Mathematics*) as elliptical for hypothetical statements bearing a set of postulates as hypothesis. Working along entirely different lines, Leonard in his paper expounds the idea that experimental and pure science are alike in their employment of speculation (framing of hypotheses) and verification. There is no difference in method, but only "a division in the 'fixed bases' [fields of fact] which these sciences treat and to which they refer for verification. On the one side [lies] immediate sensory experience, on the other usages and intuitions." Particularly good is Leonard's treatment of the difficult question, Wherein consists our knowledge of abstract systems as such?

The point of departure for Leonard's treatment of scientific method is Lewis's book, to which he frequently refers. But occasionally his sentences have a Whiteheadian ring. Also, essays which, like Quine's and Leonard's, give assurance that the authors know what they are talking about when dealing with abstract systems, are tributes to H. M. Sheffer's teaching at Harvard.

Scott Buchanan contributes an "Introduction" to the *De Modis Significandi* of the Scotist Thomas of Erfurt. Unfortunately the recently published writings of Charles Peirce have given us a standard for the discussion of signs and symbols, which Buchanan entirely fails to approach. But his piling up of words cannot be called "laborious"; the writing is facile to the point of carelessness. The conclusion of the essay is a plea for the study of grammar. Strictly speaking, however, Buchanan never pleads, but always berates. Thus he contrasts modern science with "ancient truths," and he loves to toss off derogatory references to "we moderns" which are just as irritating and no more sensible than repeated eulogistic references to "we moderns" would be. One yearns for Leonard's difficult and sometimes absent-minded but always thoughtful and definite discussion.

Paul Weiss contributes an essay on "The Nature and Status of Time and Passage." It suffers from too many distinctions which, though

pugnaciously set down and claiming to be so fundamental as to relegate to limbo all previous work on the topic in question, are in reality merely facile. Weiss's final thesis,—that to be is to be incomplete, and so to change and to persist—may be true. But instead of being proved it is approached by the announcement: "Descartes, Leibniz, Locke, Berkeley, Hume, Kant, Schopenhauer, Hegel, Einstein, Bergson, Alexander, Dewey, Bradley, McTaggart, Whitehead and their disciples—in fact, almost every philosopher and cosmological scientist in modern times—share a common assumption which is erroneous. They assume that the real objects in the universe are completed individuals at every moment of time. . . . All of them, consciously or unconsciously, seem to have embraced the principle: any individual in time can live but for a moment,"—they committed "the fallacy of essential completeness." This comes at the end of two pages in which it is easily shown that Descartes and Hume embraced this principle, and two more pages for Leibniz, Newton, Schopenhauer, Bergson, and Whitehead. Why is this principle wrong? Only one sentence of direct evidence is offered against it: "Temporal, enduring objects confront us on every side." It may safely be said that every one of these poor benighted great men knew that temporal, enduring objects confronted him on every side. It is obligatory to examine their reasons for analyzing this confrontation to produce systematic theory, instead of taking it at its face value as Weiss does. Probably it is hopeless to try to do anything on the subject of Time other than a description of time's analytic properties, unless one has made a clear formulation of what he considers the basic concepts of philosophic cosmology. The examination due the philosophers of the past would then be a lengthy task; but that affords no excuse for quick murder and a rapid climb to assurance over their dead bodies. Whitehead would require even longer examination; instead he is hastily given the patronizing condemnation, "Whitehead is thus here still one with Descartes and Hume, whom he so valiantly tried to correct."

S. Kerby-Miller's essay on "Causality" drives home a point in favor of the "intrinsic connection" theory of causality. According to this theory the causal relation holds primarily between states of consciousness, and is observable by introspection. The argument is salutary, in view of the dogmatism of the proponents of the orthodox "correlation" view. But the present reviewer doubts the wisdom of discussing causality in Kerby-Miller's terms. The introspection of states of consciousness does not lend itself to the production of systematic theory. The miscalled intrinsic connection "theory" does, however, include an important *fact*, our experience of causal relations. It is one of White-

head's greatest achievements to have lifted this fact out of its old locus for discussion, and to have made it a datum for systematic theory.

It is fortunate that Charles Hartshorne has written the only Whiteheadian essay in this volume; for he is one of the very few men whose published comments on Whitehead have shown more understanding than misunderstanding. Here he sets out the idea of "the compound individual" as the most satisfactory solution yet found for the philosophic question as to the nature of individual existence or "substance." It is an idea easily drawn from biology's hundred-year-old discovery of cells, showing organisms such as the human body containing simpler organisms as parts. Now "in the 'cell theory' or 'philosophy of organism' of Whitehead we have nothing less than the first full-blooded, forthright interpretation of the cellular model." Thus Hartshorne fastens on what is perhaps the most useful of all standpoints for interpreting Whitehead's cosmology to scientists. But the use he makes of it is too scanty to do much good; Hartshorne feels obliged to pass on to the status of Whitehead's "eternal objects" and his "God" in order to cover the scope of the idea of "the compound individual." The entire essay is too rapid in pace; subsequent elucidations of meanings can alone give its views the power of conviction. This is offset by the wealth of important statements useful for starting some badly needed discussion. In Hartshorne's pages, metaphysics seems to be really alive,—a rare treat today.[1]

Hartshorne's discussion of older theories of substance is at its best in the seven pages on Leibniz. He excellently outlines some of the reasons why, as the present reviewer believes, the great forerunner of Whitehead was Leibniz rather than Plato or Descartes or Locke, to whom Whitehead more often refers as providing the roots of his philosophy.

The collection of essays here reviewed contains enough good work to compare favorably with any of the single volumes by diverse philosophers that have appeared in English in this century; and it is more substantial than most. The fact is a tribute to the seriousness with which metaphysics and mathematical logic have been pursued at Harvard University. This seriousness goes back several decades; but the foremost factor in augmenting it during the last ten years has been the residence of Professor Whitehead in Cambridge.

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[1] Along with the present essay, N. Wiener's discussion of Leibniz and Quantum Mechanics (*Philosophy of Science*, I (1934), 479-482), should be read.

SOLID FARE OF PHYSICS

Foundations of Physics. By R. B. Lindsay and Henry Margenau. John Wiley & Sons, Inc., 1936. Pp. 537. $4.50.

This volume is frankly labelled "an advanced textbook." But it is more. It has a philosophic point of view, dwells on foundations, and furnishes the "text" that Bridgman would demand along with the formulas. The authors speak of their use of "mathematical analysis where it is essential for clear understanding," but as a matter of fact their contribution is more valuable in the contrary direction when they furnish the elucidation in those difficult spots where the mathematics is usually presented as speaking for itself. One might say one can get the mathematics elsewhere, but not the careful, critical attitude which is an outstanding contribution of these authors. Sincere workers in scientific philosophy should pay close attention to this carefully planned study which deliberately avoids the easy ways of popularization but at the same time tries not to take too much for granted. If it seems to be too difficult, a more elementary exposition may be found in *The Nature of Physical Theory* of Victor F. Lenzen (John Wiley and Sons, Inc., 1931, pp. 301). Perhaps it is not right to speak of Lenzen's work as more "elementary." It differs in being addressed more directly to philosophers and is wedded to a definite philosophy of "aspects." On the other hand its staccato style and brevity will not give one the meatiness of Lindsay and Margenau's expositions.

The topics progress from an analysis of the meaning of a physical law and of the construction of physical theories, through detailed treatments of theories, including the most recent relativity and quantum mechanical contributions, to a finale on causality.

A few excerpts can be relied to speak for themselves and to reveal the authors' approach. In a discussion of harmonics they stop to remark (p. 46): "The striking nature of the result will be appreciated when it is noted that the boundary conditions have introduced *discreteness* into the problem. (P. 50) "For us a boundary condition is any condition which limits or restricts the generality of a relation among physical symbols." Or, when they come to space and time (p. 59) "there is strictly no such thing as *space*, but only *spaces* . . . (p. 74) the use of the time parameter in physics may be looked upon as a matter of convenience and nothing more."

(P. 80) "It would be difficult for a physicist to introduce a definition of a particle which was entirely independent of and had no connection with the principles fundamental to a description of motion." (P. 128)

"is there any way of stating the general principles of mechanics using the concept of energy alone and making no *a priori* use of the equation of motion?"

(P. 252) "Quantum theory in particular indicates that something is wrong with the notion of an ensemble, for some of its states (over which Gibbs extends the averaging process) are dynamically impossible." (P. 286) "Strictly speaking, it is therefore impossible to measure precisely any field quantity."

About relativity: (p. 355) "has proved too valuable to be given up. . . . (p. 377) Rather it is likely that continual attempts will be made at minor modifications to which the theory readily lends itself (e.g., alterations in the metric of space-time, etc.)"

And here we heartily agree: (P. 394) "Indeed, many of the supposedly profound philosophical implications of quantum mechanical reasoning are inherent in classical physics and become apparent if one desires to look for them." More sweeping: (p. 398) "These three hypotheses: (1) possibility of an infinitely detailed experience, (2) identity of the elements of calculation with those of observation, (3) sharp prediction of the data of observation, are dismissed in quantum mechanics." (P. 402) "In quantum mechanics the state of a system is no longer defined by means by a number of variables having an immediate intuitive appeal and recalling exact configurations of constituent parts. In fact, it is not defined in terms of observables at all; it is simply a function in configuration space."

The treatment of causality is the sanest we have seen on the part of physicists. (P. 520) "Many physicists prefer to adhere to the classical definition of states and therefore declare causality void. Their contention is correct, of course, provided that we adopt both the positivistic formulation of causality and the classical definition of state." It seems that we can always tidy up.

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BOOKS RECEIVED

Studia Philosophica. Commentarii Societatis Philosophicae Polonorum. Curantibus K. Ajdukewicz, R. Ingarden, K. Twardowski. Volumen I. Leopoli, 1935. Pp. 498. 30 zl. A new philosophic Journal. Address: Ksiazka (A. Mazzucato), Lwow, ul. Czarnieckiege 12, Poland. Manuscripts should be sent to Prof. Dr. K. Twardowski, Lwow, ul. sw. Sofji 46, m. 6. The following contributions appear:

Die syntaktische Konnexität. K. Ajdukiewicz.
Vom formalen Aufbau des individuellen Gegenstandes. R. Ingarden.
The development of the main problem in the methodology of Francis Bacon. T. Kotarbinski.
Essai sur le théorie causale du temps. H. Mehlberg.
Der Wahrheitsbegriff in den formalisierten Sprachen. A. Tarski.
Über das Verhältnis der mehrwertigen Logik zur Wahrscheinlichkeitsrechnung. Z. Zawirski.
and numerous abstracts in French.

Wahrscheinlichkeitslehre, by Hans Reichenbach. A. W. Sijthoff's Uitgeversmaatschappij N.V. Leiden, 1935. Pp. 451. $8.

A Philosophy of Science, by Philip Eichler. G. P. Putnam's Sons, 1936. Pp. 111. $1.50.

Erster Internationaler Kongress fuer Einheit der Wissenschaft. Paris, 1935. Reprint of Bd.5 H.6, pp. 375–430 of *Erkenntnis*. Report Otto Neurath.

Time's Arrow in Society, by Anderson Woods. The University of Chicago Press. Pp. 233. $2.

SUPPLEMENT
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Postulational Methods. III

BY
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A FEW PROPERTIES OF POSTULATE SETS

WE NOW proceed to define certain terms which we shall apply to sets of axioms and derive a few properties. A great deal of what follows in this section is still based on a two-valued logic, while our criticism of the usual independence and consistency proofs is based on an n-valued logic. The necessary alterations are now being worked out by the author.

Definition. Two postulate sets are said to be prime[1] to each other if no axiom of one of the sets is an axiom or theorem of the other, and conversely, no axiom of the second is an axiom or theorem of the first.

Theorem 1. There exists at least two consistent axiom sets which are prime to each other.

Definition. By *contradictory* propositions we mean two propositions

[1] This term was first applied by H. M. Sheffer to *two postulates*, neither of which implies any part of the other. A set of postulates mutually prime by pairs he calls a set of mutually prime postulates. Cf. Abstract in Bull. Am. Math. Soc., v. 22, 1916, p. 287. We here extend the idea to two sets of postulates. All our results were reached independently, since the abstracts of Prof. Sheffer's paper merely gave the definitions.

p and q such that if p (or q) is *true* q (or p) must be *false*, and if p(or q) is false, q (or p) must be true. q is then denoted as p′ or p as q′.

Proof. Let $A_1 A_2 \ldots A_n$ be one axiom set and $A_1' A_2' \ldots A_n'$ be the other, where A_i' and A_i are the contradictories of each other.

The two sets thus defined are prime to each other, because, suppose they were not and A_j were either an axiom or a theorem of A_i' ($i = 1 \ldots n$). We would then have a) a proposition and its contradictory as axioms which would reduce the set to absurdity; in the sense that anything would follow from it b) a theorem which contradicts an axiom—which would make the set inconsistent. Therefore, the sets are prime to each other.

Definition. If A_i ($i = 1 \ldots n$) is a postulate set, then the set A_i' ($i = 1 \ldots n$) is called the *inverse set.*

Corollary 1. Any axiom set and its inverse are prime to each other.

2. Any axiom set and a subset of its inverse set are prime to each other.

Theorem 2. Any subset of an axiom set and any subset of its inverse are prime to each other.

Proof. Let A_j ($j < n$) be any subset of A_i and A_r' ($r < n$) be any subset of A_i'. We have two cases: (a) $j = r$ (b) $j \neq r$. If $j = r$ (i.e. A_i and A_j') then the two subsets are contradictories and are prime by theorem 1. Suppose $j \neq r$, i.e. A_j contains all those axioms which are not contradictory to the axioms of A_r'. Then if these were not prime, one of the axioms of A_j (or A_r') would be a theorem or an axiom of A_r' (or A_j), and therefore the set A_i ($i = 1 \ldots n$) and A_i' would not be prime. But by theorem 1 they must be.

Definition. By the *logical product* (or briefly product) of axiom sets, we mean the axioms common to both sets. E.g. if $A_1 A_2 \ldots A_n$ is one axiom set, and $B_1 B_2 \ldots B_r B_{r+1} \ldots B_n$ is the second and $A_s = B_r$; $A_t = B_{r+1}$ and for all others $A_i, A_i \neq B_j$ then the product of these two sets is $A_s A_t$ (or $B_r \cdot B_{r+1}$).

Theorem 3. The product of two prime sets is zero.

Proof. This follows at once from the definition of prime sets.

Corollary 1. The product of a set and its inverse is zero.

Proof. Any set and its inverse are prime by Corollary 1, theorem 1.

Theorem 4. If the product of two sets is not zero, then there exists an axiom set, whose theorems are theorems in both the given sets.

Proof. If the product of two sets A_i and B_j is not zero, it follows that these are not prime sets, and therefore there exist axioms common to both sets. Let these axioms be P_r ($r = s, s + 1 \ldots t$). P_r are in A_i and theorems deduced from P_r without use of the set $A_i - P_r$, are

theorems of the set A_i and also of the set B_j because P_r is in the set B_j. Likewise, any theorems proven from P_r without use of the set $B_j - P_r$ are in B_j and in A_i. Furthermore, the theorems deduced from P_r without use of $A_i - P_r$ and those deduced from P_r without use of $B_i - P_r$ are the same theorems being derived merely from P_r.

Theorem 5. A necessary and sufficient condition that two sets have their product zero is that the two sets be prime.

Proof. If the product is zero, then by definition the two sets have no axioms in common, i.e. they are prime. If the sets are prime, then by theorem 3, the product is zero.

Theorem 6. A set and any subset of its prime set are prime.

Proof. We can show this by showing their product to be zero. Let A_i be one set and let B_j be any subset of the prime set B_i'. If any axiom (say B_r) of B_j were in A_i then the product of A_i and B_i' would not be zero, because B_r would be in B_i' also. But by hypothesis B_j' is prime to A_i, therefore A_i and B_j are also prime.

Corollary 1. The product of any set and a subset of a set prime to it is zero.

Theorem 7. If, A_i is prime to B_i and B_i is prime to C_i, then A_i is prime to C_i whenever $C_i = A_i'$ (i.e. C_i is inverse to A_i).

Proof. That A_i is not in general prime to C_i when both are prime to the same set is evident as soon as we take $C_i = A_i$. However, if A_i is prime to B_i and C_i $(= A_i')$ is prime to B_i it follows that A_i and A_i' are prime by definition of A_i'.

Corollary. The relation of "prime to" is not transitive.

Theorem 8. If A_i is prime to B_i, then B_i is prime to A_i $(A_i \neq B_i)$.

Proof. By definition if A_i is prime to B_i, then A_i and B_i have no axioms in common. Therefore B_i and A_i have no axioms in common. Therefore B_i is prime to A_i.

Corollary 1. The relation "prime to" is symmetric.

2. If $A_i = B_i$ the proof fails by definition of "prime." Therefore, the relation "prime to" is not reflexive.

Theorem 9. If the set A_i contains the set B_i as a subset, and if A_i is prime to a set C_i, then B_i is also.

Proof. Suppose B_i were not prime to C_i then B_i and C_i would have at least one axiom in common. But since all axioms of B_i are included among A_i this axiom would be also. Therefore A_i and C_i would not be prime.

Definition. The *sum* of two axiom sets are those axioms of both sets, without those common to the two.

E.g. If $A_1, A_2, \ldots, A_k, A_{k+1}, \ldots A_n$ and $B_1, B_2, \ldots B_k, B_{k+1}, \ldots, B_n$

are the two sets where $A_k = B_k$; $A_{k+1} = B_{k+1}$ then the sum of the two sets will be $A_1, A_2, \ldots A_{k-1}, A_{k+2}, \ldots A_n, B_1, B_2, \ldots B_{k-1}, B_{k+2}, \ldots, B_n$.

Theorem 10. The sum of two sets, neither of which contains an axiom contradictory to an axiom in the other, is an axiom set.

Theorem 11. If the product of two sets A_i and B_i neither of which contains an axiom contradictory to an axiom in the other, is zero, then the sum of the sets A_i and B_i is an axiom set containing all the axioms and theorems of the two sets.

Conversely: If the sum of the two axiom sets is an axiom set containing all the axioms and theorems of the two sets, then the product of the sets is zero.

Theorem 12. If A_i contains B_i as a subset, then the sum of A_i and B_i are the remaining axioms, i.e. $A_i + B_i = A_i - B_i$.

Theorem 13. The product of any set and a subset of itself is the subset. $(A_i \cdot B_i = B_i.)$

Corollary. The product of any set by itself is the set. $(A_i \cdot A_i = A_i)$.

Theorem 14. If A_i contains B_i as a subset, then the sum *of the product of A_i and B_i and the sum of A_i and B_i* is A_i $(A_i \cdot B_i + [A_i + B_i] = A_i)$.

Proof. The sum of A_i and B_i are the remaining axioms of A_i. The product of A_i and B_i is B_i. Therefore, the sum of these two is A_i.

Corollary. If A_i contains B_i as a subset, then the sum and product of these two are prime to each other.

We shall now tentatively define Independence and Consistency for axiom sets and derive some properties of these. Later we shall discuss questions more fully. We first define Independence between two different sets.

Definition. Two sets are said to be *totally independent* if it is impossible to deduce any axiom of the one set from the axioms of the other set.

Theorem 15. There exist at least two sets which are totally independent.

Proof. Take any set A_i and its inverse A_i'. These two sets are the required sets, because suppose some axiom $\begin{Bmatrix} A_j \\ A_j' \end{Bmatrix}$ were deducible from $\begin{Bmatrix} A_i' \\ A_i \end{Bmatrix}$ then $\begin{Bmatrix} A_j \\ A_j' \end{Bmatrix}$ would contradict $\begin{Bmatrix} A_j' \\ A_j \end{Bmatrix}$ and the set would reduce to absurdity.

Theorem 16. Two sets are independent if they are prime.

Proof. Let the sets be A_i and B_i. By definition of "prime" no

axiom or theorem of the one is an axiom or theorem of the other. Therefore the sets are independent.

Corollary 1. Subsets of Independent sets are independent sets.

Proof. Since subsets of prime sets are prime, the corollary follows at once.

Corollary 2. Two sets whose product is zero are independent sets.

Definition. An axiom set is said to be a *self-independent*, or briefly, independent, set,[2] if for any axiom A_c, we can form a set $A'_c + (A_i - A_c)$ such that $(A'_c)(A_i - A_c) \rightarrow 0$ is false.

Theorem 17. If an axiom set is self-independent, then after replacing any axiom by its contradictory, there exists an interpretation for the resulting set.

Proof. An interpretation would be impossible to find only if the resulting set implied an impossibility, i.e. only if $A_{n-r} \cdot A'_r \cdot A_{n-r+2} \rightarrow 0$.

But this implies $A_{n-r} \cdot A_{n-r+2} \rightarrow A_r$. i.e. A_r is not independent of the remaining axioms.

This is the formal proof for the method commonly used to show independence. H. M. Sheffer believes this involves a circle, but the above proof shows no circle. We shall, however, have more to say about this later.

Theorem 18. There exists at least one independent set.

Proof. The set given by H. M. Sheffer for / serves as proof.

Theorem 19. Any subset of an independent set is an independent set.

Proof. If it were not, the complete set would not be independent.

Theorem 20. The product of two sets each independent, is an independent set if the sets are not prime.

Theorem 21. The sum of two totally independent sets, each independent, is an independent set, if no axiom of either set is the contradictory of an axiom in the other set.

Definition. Two sets are said to be *equivalent*, if the axioms of one set are derivable from those of the other, and those of the second are derivable from those of the first.

Theorem 22. There exists at least two equivalent postulate sets.

Proof. The sets for *Principia Mathematica* given by E. V. Huntington in Trans. Am. Math. Soc. 1933.

[2] Sheffer defines an *independent* set as a set of postulates such that if m is the number of postulates no m-1 of them imply the mth. Then he says that a mutually prime set of postulates imply independence. Bull. Am. Math. Soc., 22, 1916, p. 287. We do not develop the properties of primeness. Sheffer's definition is identical with ours.

Theorem 23. The sum and product of two equivalent sets, is either set.

Theorem 24. Two equivalent sets are never totally independent.

Definition. 1. An axiom set is said to be *partially consistent* if when any theorem p is deduced, it is impossible to deduce p'.

2. Two sets are *mutually consistent* if the set which results when we consider the axioms of both sets as one set is a partially consistent set.

Theorem 25. A set and its inverse are never mutually consistent. The proof follows from the definition of two sets mutually consistent.

Theorem 26. The product of two mutually consistent non-prime sets is a partially consistent set.

Proof. Since the product of two sets is a subset of each set, this subset must itself be partially consistent, or each set would not be partially consistent.

Corollary. Any subset of a partially consistent set is a partially consistent set.

Theorem 27. The sum of two mutually consistent sets is not necessarily a partially consistent set.

Proof. Let $A_1A_2 \ldots A_n$ be one set and $B_1A_2'B_3A_4A_5 \ldots B_n$ be the other. The sum of these two sets is $B_1A_2'B_3B_6 \ldots B_nA_1A_2A_3A_6 \ldots A_n$ which is not partially consistent since it contains A_2 and A_2'.

Theorem 28. The sum of two sets which are mutually consistent and equivalent is a partially consistent set.

Theorem 29. The sum of two prime sets is not necessarily a partially consistent set.

Theorem 30. If an axiom set is partially consistent, there exists an interpretation of it.

Proof. If no interpretation existed, it would mean that the set implied an impossibility, i.e. $A_1A_2 \ldots A_n \rightarrow 0$ or $A_1A_2 \ldots A_{n-1} \rightarrow A_n'$

And since any axiom set implies its own axioms $A_1A_2 \ldots A_n \rightarrow A_n$

i.e. the set by definition is not partially consistent.

Corollary 1. If $A_1 \ldots A_n$ is an axiom set, and if when any one of the axioms is replaced by its contradictory $A_1 \ldots A_j' \ldots A_n$ we can find an interpretation then $A_1 \ldots A_j' \ldots A_n$ is a partially consistent set.

2. If an axiom set A_i is an independent set, then the sets formed by replacing each axiom by its contradictory (i.e. $A_1'A_2 \ldots A_n$; $A_1A_2' \ldots A_n$ etc.) are partially consistent sets.

This shows how the problem of consistency can be referred to that of independence and conversely. H. Weyl in *Philosophie der Mathematik* (p. 18) has put the matter in more metaphysical

terms. He says that a scientific theory cannot yield a real contradiction to the given. Therefore, the proof of consistency by representation is a real control. There are, however, certain difficulties involved in both these concepts which we shall discuss later. These are due to the failure to note the distinction between partially consistent and totally consistent sets. Cf. our definition of consistency later.[3]

Although we are quite conscious of the fact that we have used a method of proof which Brouwer has rejected, we have not made use of our analysis of Brouwer for these reasons (a) we are dealing here only with a finite set of axioms; (b) even re-interpreting our results to read "possible" for "true" wherever we have used the law of excluded middle, will still retain all of the properties derived even if in different form.

INDEPENDENCE PROOFS. CRITICISM AND ANALYSIS

We have already shown in *theorem 17* of a previous section that a necessary and sufficient condition for showing independence of a set with one of its postulates, is to find an interpretation of the set with the contradictory of the postulate. In *theorem 17* we only proved part of this, but the converse is easily established. The first attempt at a proof of necessity and sufficiency in verbal form was given by *A. Padoa* at the *International Congress of Philosophy* at Paris in 1900. But Padoa recognize that there were certain difficulties involved when he points out that the failure to prove a system independent proves nothing at all. As a consequence we see that despite the fact that we do have a necessary and sufficient condition, we have no mechanical means for solving the problem directly in any given case. Such a solution will be at hand if we use a method based on the logic of H. B. Smith (which we explain later in this section) and is contingent upon the solution of the general problem of logic referred to previously.

Illustrations of this usual method of showing independence can

[3] For other properties of postulate sets and the definition of further concepts cf. "Axiomensysteme für beliebige Satzsysteme," Teil I, "Sätze erster grade," Teil II "Sätze höheren grades." Math. Ann. v. 87, 1922, p. 246 ff. and v. 89, 1923, p. 76 ff. This is so far as I know the only other work on this topic.

be found in every article which sets up postulate sets. One such has been given previously and others can be found in articles by Sheffer and Huntington.

Despite the fact that in an interpretation we appear to have a necessary and sufficient condition for independence, a great deal of dissatisfaction has been expressed with this method. Most of it is based on the fact mentioned previously that the failure to find such an interpretation proves nothing. Thus for example, R. M. Eaton says "The Proofs of Independence, consistency and completeness usually employed are thoroughly unsatisfactory; they involved an appeal to interpretations. Suppose that the system has no interpretation, how can the consistency, independence and completeness of the postulates be shown? There should be some analytical way—purely in the realm of the abstract without interpretation—of establishing these properties of a set of postulates." (General Logic 1931, p. 474n). The first part of the quotation shows a misunderstanding. *If it can be shown* that a set has no interpretation, then that set is not consistent, and also another set with an axiom contradictory to one in the original set is not independent. This is due to the fact that this interpretation is a necessary and sufficient condition. But notice, and this is the important fact, that in order to show that a given system is not independent, we must offer an *impossibility proof*, i.e., *a proof that an interpretation is impossible.* Too frequently we hear that such and such a thing has been proven impossible, only to have the impossible accomplished immediately afterwards. As far as possible, we must avoid such proofs and attempt to find purely analytical tests.

The two important objections, then, to this type of independence proof are (1) if no interpretation is found we have proven nothing; (2) to show a system non-independent, we become involved in impossibility proofs.

We are compelled then to consider this necessary and sufficient condition to be either too general or too special. In either case we must find either another condition, both necessary and sufficient or else an equivalent condition, less general or more general as the case may be, which can be applied. We shall indicate the

way to such a condition later after consideration of what has been termed "complete independence." But we cannot deny that an interpretation is a necessary and sufficient condition, since it has been proven so for *ordinary* implication.

When we wish to show a set of axioms $A_1\ A_2....A_n$ consistent, we seek an interpretation for these axioms. When we wish to show A_i independent of $A_1\ A_2....A_{i-1}\ A_{i+1}....A_n$ we replace A_i by A_i' and seek an interpretation of the set $A_1'\ A_2....A_i'...A_n$ i.e. we show this set to be a consistent set. In seeking constants for our variables, we use either concepts of other systems or entities of the physical world. In the first case it is assumed that the system used is itself consistent, and in the second case it is assumed that "reality is consistent." Thus for example, in order to prove Euclidean geometry independent of the parallel axiom, we deny that axiom, and show that the resulting set can be interpreted on a pseudosphere. It is, of course, assumed that since we can do this, we have referred the matter to the consistency of reality. Or again, it was seen that Sheffer referred the independence of his axioms to the consistency of smaller sets which he defined. In either case neither independence nor consistency is proved, but merely referred back to another system. Notice, however, that this criticism does not deny the condition to be necessary and sufficient, but merely points out difficulties in the application of the condition.

Clearly what must be done is to show in a purely analytical fashion that an interpretation exists if *any* consistent system exists.

If we recall certain methods used in the Theory of Infinite Series in mathematics a possible solution, other than that suggested below, is opened to our consideration. We recall that in order to prove that a given series converges or diverges, we compare it with another series known to converge or diverge. This second series was shown to converge or diverge directly. The test is as follows, and can be found in almost any book on Algebra or Series; if a series of numbers is given such that every term is less than every term of a series known to converge, then the given series converges:—if every term is greater than every term of a

series known to diverge then the given series diverges. The important fact to be noted is that certain series are known to converge or diverge by various means.

Now, suppose we have a set of axioms which are finite in number and from which only a finite number of theorems can be drawn. We can show such a system consistent or inconsistent immediately by deducing the complete system. In this way we can obtain systems known to be consistent. If we have two such systems shown to be consistent, we can obtain a greater system by combining these two. The resulting system being finite also can be seen to be consistent or inconsistent by direct investigation.

Given any set of axioms which we wish to show independent, we interpret the variables in terms of one of the systems already shown to be consistent. If the resulting propositions are all true theorems or axioms of the set known to be consistent, then these offer an interpretation and we have shown independence. It is necessary to have as many systems known to be consistent as there are axioms.

For the case where we have an infinite number of propositions to be shown independent we have a much more difficult task and one which can only be solved with the solution of all true and false propositions deducible from the given set. It is true that this case will practically never occur, but it must be considered if we are to be able to say our solution is general. At any rate, it is a possible case unless it can be shown that no infinite set of propositions can be an independent set. If we mean the infinite set of all propositions, (which is one of the paradoxical sets) then clearly that set cannot be an independent set (i.e. if it exists abstractly it cannot be) since it contains propositions and all propositions deduced from them. There are then different infinite sets of propositions. Abstractly considered, the set defined by the property that no one of its elements can be deduced from any one or any combination of the others, is a possible set.

In order to show such a set to be an independent set we must either show that *any* axiom of the set except the one in question, satisfies the interpretation or that *any* sub-set of the set is an independent set. In the first case we note the structure of the

axioms and show that they must satisfy the interpretation. In the second case we show that *any* sub-set is independent and further that any two such sub-sets are mutually independent and therefore their combination will be an independent set as proven above.

In most cases independence has not really been solved, but the question passed from one system to another. Each system set up to prove independence for each particular axiom is assumed consistent. Very frequently a circle does arise. The set A is shown consistent by reference to system B which is itself shown consistent with reference to system A.

It is obvious in how close a connection independence and consistency are. O. Holder offers an excellent illustration for the case of the parallel axiom, of how independence is proven, by reference to the consistency of Lobatschefski's non-Euclidean Geometry. The problem of the Independence of the parallel axion of Euclid, is the most famous problem of all. As is well known many attempts were made to deduce the axiom—since it did not appear to be as self-evident as it should. Not the least of these attempts is Saccheri's "Euclides Vindicatus" and in the introduction to the translation made by G. B. Halsted it is remarked "Saccheri was the first to use this procedure (i.e. of construction of examples) and constructs his example without leaving the field of formal logic" (Open Court 1920 p. XXI).

However, it appears upon closer examination that Saccheri's attempt was related rather to the inconsistent triad than to an actual interpretation as discussed just above. This method is actually the method of indirect reduction which is denoted by the letter "k" in the mnemonic names *Baroko* and *Bokardo* of the Aristotelian logic. The name antilogism was given to this by Mrs. Ladd-Franklin. Briefly the procedure is that if a given proposition is implied by other propositions, to deny it and combine it with some of the other propositions should lead to a contradiction of a known truth. As an example, consider the mood Baroko. This is AOO in the second i.e. A(ab) O(cb) implies O(ca). In this we assume that A(ab) and O(cb) are to be taken true, i.e. they might be considered as the axioms of the system. Now the

question is, does O(ca) follow as a theorem from these two? If it does not, i.e. if it is independent of A(ab), and O(cb), then A(ab) O(cb) should imply A(ca). But if we consider A(ab) and the theorem A(ca) together we have a mood in Barbara implying A(cb). But A(cb) contradicts O(cb) and since in our implication A(ab) A(ca) implies A(cb) A(ab) was given true, it follows that A(ca) cannot be true, i.e. to assume O(ca) is independent of A(ab) and O(cb) leads to a contradictory system. Therefore, O(ca) is not independent of the others. As an illustration of this method to other propositions one can use the following three propositions:

1. All men are mortal
2. All lawyers are men
3. All lawyers are mortal

The application of this method to independence proofs is obvious from the discussion. The objection which occurs is again the clumsiness of the method. For the case (which is rare) in which we *can* deduce all the theorems of the system with the contradictory of the axiom in question this method is applicable. But, as in the case of Saccheri, where this is not possible we only have a probable consistent resultant system unless we can show the contradiction at once.

But in the last analysis this method is merely another unwieldy form of our necessary and sufficient condition as stated above. In its general form the inconsistent triad is as follows:

If p q r ... implies T, then by contradiction and interchange p q r...T′ implies a contradiction (where T′ indicates the contradictory of T), and that is the condition stated previously.

Symbolically the law is as follows: $pq \rightarrow r : \rightarrow : pr' \rightarrow q'$ which is really contradiction and interchange and can be derived using

1. $p \rightarrow q \cdot \rightarrow p' + q$
2. $p + q \equiv q + p$
3. $(pq)' \equiv p' + q'$

Proof.

$$pq \rightarrow r \cdot \rightarrow \cdot (pq)' + r \text{ (by 1)}$$
$$\cdot \rightarrow \cdot p' + q' + r \text{ (by 3)}$$

$$\cdot\rightarrow\cdot p' + r + q' \text{ (by 2)}$$
$$\cdot\rightarrow\cdot (pr')' + q' \text{ (by 3)}$$
$$:\rightarrow: pr' \rightarrow q' \text{ (by 1)}$$

taking one step further, we have.

$$pq\cdot\rightarrow\cdot r:\rightarrow: pr'q \rightarrow o$$

In the preceding discussion no attempt was made to analyze more closely the definition of Independence. In any set of propositions there are involved a set of concepts, a set of relations affirmed to exist between these concepts and a set of properties. As a result, we must consider briefly what may be called "Intensional Independence." If two concepts are entirely unrelated then, of course, the two propositions which involve them must be independent. If it can on the other hand be shown that two concepts are derivative, one from the other, then the propositions which affirm or rather enunciate their properties must be derivative, and hence non-independent. For example, if we accept Whitehead and Russell's doctrine that the concept of implication is definable in terms of disjunction, i.e. implication can be derived from disjunction, then any theorem relating to implication must be derived or derivable from those of disjunction. In fact many of the theorems of *Principia* about implication are proven by reducing the implicative relations to disjunction. As a consequence, implication is non-independent of disjunction. Only one of these two can be taken as a primitive idea. Two axioms are independent if a new concept occurs in one which is not in the other,—independent in the sense that one cannot be derived from the other.[4] On the basis of this it follows that two propositions, one of which contains a concept contradictory to a concept in the other, are independent, but not of course consistent.

Taking the set of elements in any set of propositions, we can have the following cases:—1) the set of propositions affirm of the same concepts,[5] the same relations; 2) they affirm of the same

[4] Cf. Weyl "Handbuch der Phil. der Math," p. 18.

[5] We mean by concept here the subjectival concept as opposed to the adjectival and relational concept.

concepts different relations; 3) they affirm of the same concepts the same properties; 4) they affirm of the same concepts different properties; 5) they affirm of different concepts the same relations; 6) they affirm of different concepts different relations; 7) they affirm of different concepts the same properties; 8) they affirm of different concepts different properties.

Clearly in the cases 1) and 3) we do not have independent propositions. In so far as the same concepts must have the same properties 4) is impossible, if however two or more propositions affirm various properties individually, they are equivalent to one proposition. The remaining cases must be separately considered.

To affirm of the same concepts different relations gives two cases a) the relations are independent; b) the relations are derivative. In b) we have the case discussed above for implication, and the propositions are therefore non-independent. In a) we have what are called "external" relations and the propositions are independent. As an illustration: given the concepts a, b, and the relations $\sim$ and $+$, then the propositions

$$\sim\sim a = a$$
$$a + b = b + a$$

are independent. This takes care of 2).

5) Is seen to involve dependence immediately, since by generalizing the propositions we can derive all the given propositions. As an example; given the different concepts *a*, *b*, *c*, *d* and the relations R, R′, and *a* R *b*, *a* R′ *b*, *c* R *d*, *c* R′ *d*, we can by using variables x, y write: given the concepts *a*, *b*, *c*, *d* and the relations xRy; xR′y. The four propositions then become theorems.

6) Involves independence immediately.

7) Means either the properties enunciated are not all the properties, or else the concepts are not different. If the properties are not completely stated, it follows that the concepts are derivative under limitations.

8) Involves independence immediately.

To set up a logic of independence then based upon intensional factors, would involve a consideration of the eight cases men-

tioned above. We do not intend, however, to undertake that task here, since we are concerned with purely formal independence, which is a prior question.

Although we cannot say that if two concepts can be attributed to the same object they are derivative, we can say that if two concepts cannot be attributed to the same object they must be incompatible. Hence it follows that given two concepts, if we can attribute them to the same object, they are compatible. It follows, further, that in the ordinary proof of independence by interpretation we demonstrate at the same time the compatibility of the concepts involved.

The underlying assumption in interpretations which makes an appeal to the experiential world is that contradictory attributes cannot exist in the same objects. It follows from this assumption that if we can give such an interpretation of our propositions we have been successful, because the propositions involved are compatible or consistent. This assumes the validity of the law of contradiction, and of course involves the metaphysical doctrine of Hegel "The real is the rational."

Returning now to our definition of independence, we note two cases:—1) a proposition p is independent of a proposition q if p cannot be deduced from q and q cannot be deduced from p. Both aspects must be taken into consideration, since we would not call a proposition p independent of q if p implied q but q did not imply p. The other two possible cases are cases of dependence also. 2) where we have a set of propositions, $A_1 A_2 \ldots A_n$ no A_i must imply any other A_j ($i \neq j$ nor must any combination of any number of propositions imply any one of the propositions.[6] The terms irredundant, irreducible, and completely independent have also been used for independence. In view of the fact that these two cases are possible, it is advisable to use the term Irredundant[7] for the second case.

Material Implication must, as has frequently been asserted, be ruled out of consideration. If we allow material implication

[6] Cf. Lewis and Langford *Symbolic Logic*, p. 337.

[7] Cf. A. Church, *On Irredundant Sets of Postulates*. Trans. Am. Math. Soc., 1925, p. 318 ff.

the whole problem becomes meaningless,[8] since any proposition is implied by a false proposition, and a true proposition is implied by any proposition. Thus as Professor Lewis points out, two propositions can never be independent and consistent at the same time. But from what has been said, our entire concern is to find a set of propositions which are both independent and consistent. We are faced then with the alternatives: either we deny the validity of material implication, or admit it doesn't apply in this case. It is quite possible that in postulate sets we are concerned not with implication, but with *deducibility*, but are we then to admit that if we are dealing with a set of propositions (not necessarily axioms) we cannot speak of their independence and consistency? It becomes clear that logic is overlooking certain distinctions which must be made.

In consideration of the second case mentioned above we are faced with the possibility that although in a set of postulates we may have independence in pairs, the set may not be completely independent. This seems to have been recognized by E. H. Moore. In order to solve this problem Moore makes certain remarks which will lead us later to a solution.[9]

Any proposition p divides the universe into two compartments —p and p′ (not −p). Two propositions p and q would then divide the universe into the following compartments: pq, p′q, pq′, p′q′

[8] In an article in the Monist (v. 29, p. 152 ff.) Lenzen shows that the only set of postulates which are independent is one of *only one* postulate. The proof is based on the assumption that $q \cdot\rightarrow\cdot p \rightarrow q$ (2.02 of *Principia*). He reduces Huntington's sets of independent postulates to non-independence as follows for A_2 and M_5

$$A_2\ (a + b) + c = a + (b + c)$$
$$M_5\ a \times b = b \times a$$

Proof

$$a \times b = b \times a :\rightarrow: p \cdot\rightarrow\cdot a \times b = b \times a$$
$$\therefore p \cdot\rightarrow\cdot a \times b = b \times a$$
$$\therefore (a + b) + c = a + (b + c) :\rightarrow: a \times b = b \times a$$

This can obviously be done for all postulates in any set.

[9] Cf. E. V. Huntington's discussion in *Postulates for Betweenness*, Trans. Am. Math. Soc., 26, 1924, p. 277.

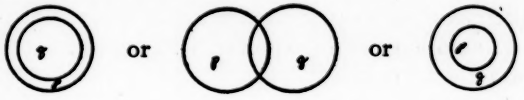
(a)

	p	p′
q	pq	p′q
q	pq′	p′q′

In the case of three propositions p, q and r, we have pqr, pq′r, pqr′, pq′r′, p′qr, p′qr′, p′q′r, p′q′r′. These can be represented by intersecting circles. Suppose all of these regions exist, then there is no relation of implication between them. If any one does not exist, any pqr′ → o, then we have pq → r, or pq and r are not completely independent. In general, we can say that if we have n propositions, we shall have 2^n compartments. If any one of these 2^n compartments do not exist, there is a relation between the propositions involved. This gives rise to the doctrine known as the "complete existential theory."

It is to be noticed that if any region in the universe represents an impossibility, we have non-independence, since the region o.pq′ → o i.e. an impossibility implies anything. This is different, however, from saying that a false proposition implies any proposition; since p′q′ is not necessarily zero. This shows clearly how material implication which fails to make necessary modal distinctions must lead to strange results. If falsity = impossibility, then what Lewis says about independence and consistency in this type of implication holds. But Brouwer and Smith have clearly seen that this is not so—modal distinctions must be made.

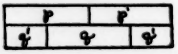
The method outlined is, of course, another approach to the necessary and sufficient condition for independence given previously. It expresses in diagrammatic form the same condition. *This is not the same as ordinary implication.* If p implies q, the relation can be represented as



(a)

as will be seen below. However, with the introduction of other propositions, we have always the possibility that these spheres will intersect and allow of implication. It ought, however, to be possible to reduce the latter case to the former. E. G. if pq′r′ does not exist, then of course pq′ → r i.e. the intersection of pq′ should contain r or be contained by r. At any rate, we ought to come out the same way.

If we represent the four compartments as we have done above, it is possible that we are overlooking a very important case. Suppose we use a form we shall use later:

(b) 

where these horizontal bars are to be considered to lie one atop the other. We notice that (pq → o)′, (p′q′ → o)′, (pq′ → o)′ and (qp′ → o)′—yet there is an implicative relation of some sort between p and q; between p and q′ between p′ and q′ since they do overlap. In other words:—if pq′ actually vanish, (i.e. pq′ → o then p → q because q contained all the p, since that is the only way pq′ → o. By reference to the diagram (a) above, this will seem to be so. By reference, however to diagram (b) it is evident that if pq′ vanishes, then p is the q. With the addition of more propositions the situation becomes more and more complicated, and it becomes necessary to find a reduction formula. It is evident that the method outlined by Huntington for complete independence is not valid, since it considers only a special type of diagram, i.e. a special case of overlapping. The method as stated, can still be used for consistency despite its speciality. It is necessary only to demonstrate the existence of a compartment, to show the consistency of the propositions whose intersection this represents.

This discussion substantiates our previous remarks that proving a set pq.....r′ to have an interpretation shows the consistency of pq....r′ and therefore the independence of pq.....r. The failure to do so tells us nothing. *We must conclude, therefore, that the condition for independence which was said to be necessary and sufficient is so only for a special type of implication, namely that in*

which p is in q or q is in p. In the case of diagram (b) if $pq' \rightarrow o$ then p does imply q *but if* $p \rightarrow q$ *it does not therefore follow that* $pq' \rightarrow o$. Some negation of q with p vanishes, but this question is not to be discussed here. It appears that certain other modal distinctions are necessary which have been overlooked. We have shown that when (b) occurs, we do not have independence for all meanings—i.e. there may be some meanings for which p does imply q.

About a year after Huntington's article cited above, a paper appeared in the Transactions of the American Mathematical Society[10] by A. Church entitled "On Irredundant Sets of Postulates." This seems to be an attempt to remedy the situation just described by shrinking some of the postulates until total inclusion or exclusion results. We outline this paper in part.

In classic logic we know that if e.g. A implies I, that A is said to be a *strengthened* form of I and I a *weakened* form of A. If we have *any valid* implication then the antecedent is the strengthened form while the consequent is the weakened form. This distinction Church adopts when he says that if p implies q then q is said to be weaker than p. Given a set of postulates $A_1\ A_2.....A_n$ then A_1 can be weakened with respect to the set A_i ($i = 1.....n$) if there exists a postulate A_k, weaker than A_1 such that the logical product of $A_k\ A_2....A_n$ implies A_1.

A set of postulates is then defined to be *irredundant* if the postulates are independent and no one of them can be weakened with respect to the set.

We see immediately that if a proposition A_i can be weakened with respect to the set, then that set cannot be an independent set, i.e. at least one axiom can be derived as a theorem. Suppose A_1 implies A_k and the set $A_1\ A_2.....A_n$ is weakened to $A_k\ A_2....A_n$. This means that $A_k\ A_2....A_n$ implies A_1. But A_1 implies A_k Therefore $A_k\ A_2....A_n$ implies A_k. Therefore A_k can be derived. from a theorem and is therefore a theorem.

Church then goes on to indicate the relationship between *complete independence* of Moore and this relation of Irredundancy.

[10] V. 27, 1925, p. 318 ff.

If we represent the set of all possible mathematical systems by a square, a postulate is then an inner square which divides the square into two regions, one satisfying the postulate, the other not.

The shaded square represents the region of sets satisfying the postulate. Call this region B.

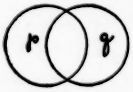
Then if we have a postulate B such that A is weaker than B, A would lie within this larger region B. This is the ordinary idea of implication.

We proceed to our own analysis.

Before doing so we wish to state that proceeding as we must on the basis of a logic still not general, at least with respect to negation, our methods here are only tentative.

If two propositions are in dependence, they may be represented in two ways

1.

2. 

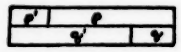
Case 2 shows those instances for which a proposition implies another only for *certain* values. Case 2 may be represented as:

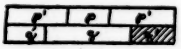
i.e., p may intersect q in more than one region. However, this reduces to Case 2 as follows; we can either break up p into two parts p_1 and p_2 having no common values, and retain both, as e.g.

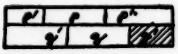
or else we can throw away one part of p as already contained in q, namely that part of p which is in q on one side or the other, as e.g.

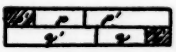
It is not necessary that the regions be well-behaved. They are represented in the way they are merely for convenience,—the important thing being their intersections.

Having reduced intersections in this way, we shall represent them in a different fashion as follows[11]


Case 1


Case 2


Case 3


Case 4

The upper horizontal line is to be conceived as lying entirely on the lower, and the line is to be considered the universe. Thus in case 1, q is entirely within p. In the case of more than two postulates we merely add another line above these, taking into account all the possible combinations and making the reductions indicated in case of a repetition of intersections. Thus the shaded parts in case 2, 3, and 4 can be reduced as described above.

Case 4 is the only case that can properly be called independence. In all other cases the propositions are related in such fashion that at least for one value there is an implicative relation. We call case 4, total independence; case 3, partial independence; cases 1 and 2 non-independence. In cases 3 and 4, it is noticed that pq', $p'q$, (case 3 only) and $p'q'$ are none zero. In case 1, $p'q$ is zero, and in case 2, pq' is zero.

[11] Cf. H. B. Smith *Abstract Logic or the Science of Modality*, Jan. 1934 (privately distributed manuscript).

This suggests the possibility of the following case which is also

Case 5

not a case of independence.

We tabulate our results

Case 1	$p'q = 0$
Case 2	$pq' = 0$
Case 3	none zero
Case 4	$pq = 0$
Case 5	$p'q' = 0$

As long, then, as we have the condition that p and q may overlap even in part, we cannot speak of total independence. E. H. Moore's *Complete Independence* and A. Church's *Irredundant Sets* are only cases of partial independence, and hence their methods must be inadequate. They base their systems, as have all attempts to define independence, on the incomplete disjunction "either Case 4 or Case 2 are alone possible." As a consequence, if Case 4 is shown to be invalid, Case 2 is left. It is now evident that Case 4 may be invalid, and we may *not* have Case 2. This is really the heart of the argument we directed against the methods outlined above.

The argument for independence was as follows:—if p implies q, then $pq' = 0$. Therefore if $pq' \neq 0$ p does not imply q. Applying this to Cases 2 and 4, it holds. But in Case 3 for example, $pq' \neq 0$, and for *some* values of p, p does imply q. Independence proofs as we said before are therefore invalid and new ones must be developed.

Now given a set of postulates $A_1 \ldots A_3$, we shall say they are totally independent only if we have

i.e. their spheres do not overlap at all. There are no values which satisfy any one which also satisfy any other. As a result, in order to prove a set totally independent, we need only prove the postulates so independent in pairs. (We recognize that this involves certain difficulties with respect to consistency—the solution of this will be given below.) In order, however, to draw inferences from the product of any combination, it will be necessary to weaken the postulates with respect to the set. Having shown that the postulates are totally independent in pairs, we can weaken the postulates and get a set independent in the usual sense. We must, however, take care not to weaken any postulate so much that its new form embraces entirely any of the other postulates. The converse operation is also possible. Having given a set which has been shown to be independent in the sense that a system exists satisfying all but one of the postulates, we can then go about strengthening the postulates until we have a totally independent set. If this cannot be done, it is because at least one of the postulates lies wholly within one of the others, i.e. is not independent at all.

We can state the condition (a) for *total* independence as follows: if $A_1\ A_2 \ldots\ldots A_n$ are the postulates, then $\pi\ A_i\ A_j = o$ $(i \neq j)$ $(i, j = 1 \ldots n)$ (π indicates the logical product)

(b) for *partial* independence if A_i implies A_{k_i} then under same conditions

$$\pi\ A_j\ A'_{k_i} = o \quad (A'_{k_i} \text{ means contradictory of } A_{k_i})$$

We would be compelled at this point to end the discussion and await the complete development of Logic were it not that Smith's system of Logic is well on its way to completion and offers us a sign-post of the direction of advance. As a consequence we shall indicate certain portions of his Logic and show a formula which gives us so far as we know a method for the solution of this problem. Its application will be given in connection with the problem of the next chapter. It must be remarked that the complete use of this formula involves a knowledge of the atomic elements of Logic as well as those of the system to which it is applied.

The general problem of Logic is to set up all true and false

propositions involving any number of implication symbols. In order to do this it is necessary to have a means of reducing any complex of propositions to a sum of products of modal functions. This formula Smith has given as

1. $|pq| = pq + p'q'|p||q| + |p||q'|' + |q||p'|'$.

However it was shown that this does not hold in certain cases. It is precisely for those cases that we do not need this formula as a condition for independence.

We define

Def. 1. $|p| = (p \to o)'$ which may be read as *p is possible*; $p \to o$ is to mean that *p is impossible*. $(p \to o)' =$ *it is false that p is impossible*. $|p|' = p \to o$ read *p is impossible*.

By $|p|$ we mean that there exist values which satisfy p.

$||p|| = [(p \to o)' \to o]'$ and may be read *it is possible that p is possible*.

p by itself means *p is true*.

Postulate $p \to (p \to o)'$ i.e. $p \to |p|$

p' means there exists at least *one* value such that p is not satisfied. This is to be distinguished from $p \to o$ which means that there are *no* values for which p is satisfied.

As a consequence p' is consistent with $(p \to o)'$. Thus the formula can be read as follows

p and q are possible together means either p is true and q is true, *or* p is false but possible, and q is false but possible, *or* p is possible and it is impossible that q be false, or *q* is possible and it is impossible that p be false.

Example. I and O are possible together means *either* I and O are true together, *or* I is false but possible and O is false but possible, *or* I is possible and it is impossible that E be true, *or* O is possible and it is impossible that E be true.

By contradicting 1, we can obtain after reduction

2. $|pq|' = |p|' + |q|' + pq'|p'| + qp'|q'|$

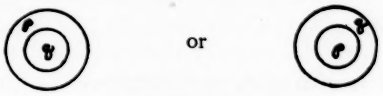
the expansion for p and q being impossible together.

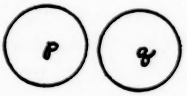
Recalling now that we defined *partial independence* as meaning

the existence of values in common and *total independence* as the existence of no values in common, we can interpret 1 and 2 as follows.

In order for two propositions to be independent *totally* they must be impossible together, i.e. | p q |′ must evaluate to *one*, or | pq | must vanish. Therefore given two propositions (say A and E) if when we substitute in 1, the resulting form vanishes, then we say that *A* and *E* are impossible together, i.e. A is completely independent of E. Note that we are not using precisely the same concepts that Smith uses—although perhaps they amount to the same in the end.

These formulas have been shown to break down for the cases where p and q are related as

(a) 

(b) 

i.e. when the formulas fail then we have cases (a) or (b), but the converse is not true, when we have cases (a) or (b) the formulas do not always break down. As a result, we can say that when given two propositions p and q, the formulas fail to be either zero or one, then we have a case of ordinary implication, (i.e. non-independence) or total independence. Other considerations would then be necessary to determine which of the two cases we have. But we feel that this breakdown is due to the application of the formula to ordinary implication. If we interpret the matter in terms of independence, we have a temporary tool which can be used in many cases. But even for the case of implication, the formulas can be handled comfortably even though necessitating some amount of work, as Smith shows in "Abstract Logic." (Cf. below where a general method of independence is given on the basis of the above formulas.)

CHAPTER 8. CONSISTENCY PROOFS. CRITICISM AND ANALYSIS

In a lecture held at Zurich in 1932, P. Bernays indicated three types of consistency proofs.[12] These were 1) The methods of evaluation 2) The methods of "Ausintegrierens" (as illustrated in the methods of Lowenheim and Skolem) and 3) The methods of Eliminations. 1) Is the method used by Hilbert and illustrated in chapter 1.[13] This is weakened because of its dependence upon a reference to other systems assumed to be consistent. 3) Is really another form of 1) and hence reduced the three methods to two.

A great many of the criticisms we directed against independence proofs hold for consistency proofs. Most of the usual definitions and proofs of consistency for a set of postulates depend upon the consistency of some other sphere. The usual definition of consistency has been that two or more propositions are consistent, if there exist values which satisfy them all simultaneously. Again if we accept the consistency of Reality we are forced to argue in this fashion:—the Real is consistent, therefore that which can be attributed to the Real *under similar circumstances* must be consistent. Hence if given a set of propositions we can interpret them in terms of Reality, they must be consistent. However, since Logic expresses or attempts to express relations as formal as possible, it must be the aim of Logic to find, as for the case of Independence, purely formal conditions for consistency. We should be able given any propositions or *propositional functions*, to determine from their form alone, whether or not they are consistent.

It may be objected, that despite formal consistency the concepts involved may be inconsistent.[14] This, however, overlooks a very important fact.

In general the concept appears to determine the form of the proposition into which it enters. If, for example, our concept

[12] Cf. Verhandlungen des Intern. Math. Kongresses Zurich, 1932, p. 342–343.

[13] Another good illustration is in Veblen and Young's "Projective Geometry," p. 2–3. We do not reproduce it here.

[14] Carnap in the Abriss der Logistik sets up two columns, one for concepts and one for propositions (p. 70). We show this to be unnecessary.

is a class-concept, the form of the proposition into which it enters will be of class inclusion or exclusion—i.e. A, E, I, O, U, V; which Smith has shown in his work to be necessary and sufficient to express the relation of classes to each other. If, the concept is a relational one, then the proposition into which it enters will be of the form xRy. Now if we have given two concepts which are contradictory, and desire to use them the following will happen. We express these concepts in propositional form and use these propositions. Obviously, we do not use concepts except in such form. Since these concepts determine the form of the propositions they will either 1) determine forms obviously contradictory or 2) determine forms contradictory but not obviously so.

Another argument is as follows. Concepts in and by themselves, apart from any other concepts, are neither consistent nor inconsistent with these other concepts. It is only when these concepts are related in some way to other concepts, that they can contradict. But concepts are related to other concepts only in propositional form. Therefore, if the concepts are contradictory, the propositions which express the relations between these concepts will be contradictory. As a consequence, it follows that all we need show is formal consistency or inconsistency, since this includes "intensional" consistency.

Of course, we must mention that it may happen that the concepts are consistent while the propositions they enter into are not. This case is very easily recognized and reduces immediately to formal inconsistency. It is therefore not important to our considerations.

Despite the fact that consistency is a very vital property of a postulate set, very little attempt has been made to solve the problem in general. For the most part only the properties of the ordinary methods were investigated. Thus for Carnap a consistent set of axioms is a "fulfilled" set, i.e. one which has a model (Erkenntnis Vol. 1, p. 302); and for Hilbert existence = consistency; while for the Intuitionists consistency is existence, but existence means constructability. As far back as 1771 Lambert in "Anlage zur Architektonik" (p. 16) had already said "The possible is what contains no contradictions." In all these cases

the consistent set of propositions has an interpretation or model. Having such an interpretation, it is argued that it will then be impossible to deduce a proposition p and the contradictory of that proposition at the same time. Here again the assumption is that if a set of properties can be attributed to a system that system is an exhibition that the properties are consistent and as a consequence, since contradictory attributes cannot belong to the same object, we can never get contradictories from the set of properties. This method, depending as it does upon finding a model, must therefore be considered weak.

Since the definition of consistency involves "implication"—e.g. pqr ..... are consistent if when pq .... imply x, it is impossible that pq ..... imply x′—the close relationship between this problem and Logic becomes evident. In fact, it is an amazing situation that this has not been recognized earlier. Fraenkel[15] suggests that the problem must be referred to Logic ultimately. He feels this to be a very difficult task because of a possible circularity, due to the fact that it will necessitate a proof of the consistency of Logic, which he claims is not less problematic than mathematics. However, granting an absolute proof of consistency possible, such a proof must use logical principles, i.e. it must assume a system of logic already consistent. So we seem to be led either to a circle or to a vicious infinite regress. This again overlooks the nature of Logic.

Suppose we were asked to define a "square." Upon saying that a square is a plane figure having four sides, the question is raised, "Is this definition 'square'?" Obviously a nonsensical problem. The real problem to be raised is—*Is this definition a definition of "squares?"* And this can only be answered by applying it to the objects supposed to possess that property. So Logic is, in its general form, a *definition of consistency*, and therefore it is meaningless to ask whether Logic is consistent. The way to answer the problem—*is this a definition of consistency?* is to apply it to those objects supposed to have that property—namely, sets

[15] Cf. Einleit in die Mengenlehre, p. 365.

of propositions. As a consequence, if given a set of variable propositions, we can interpret them in logical terms or attain a logical model, we do have a consistent set of propositions. Or else we apply a method to be given below which determines the implications between propositions under certain conditions. It is to be noted that the definition of consistency

$$p \text{ cons. } q \equiv [(p \rightarrow q)\ (p \rightarrow q')]' [p \neq o]$$

is itself a logical principle. But note below the definition we use which avoids the difficulties in this.

To the above two objections might be raised: 1) Logic itself is in a very doubtful state: 2) the objection of Fraenkel that Logic uses Number-theory and Theory of Aggregates, and cannot therefore be used as a consistency proof without a circle. To the first objection we answer, that all that signifies is that a consistency proof would still be impossible if Logic were not developed far enough, since we would not have all the conditions of consistency. To the second we answer, that such an objection is untenable. Logic does use the concept of class and of number, but these are logical concepts fundamentally and are therefore defined in Logic or taken as the undefined elements of Logic. Mathematics merely develops these concepts in their application to other objects. There is, therefore, no circle involved. Furthermore, although it is true that Mathematics is not the particular of which Logic is the general, yet in a very real sense Mathematics does and must use the principles of Logic. Although there are common factors, it is not correct to say that Logic involves certain branches of mathematics as mathematics involves Logic. The principle of the Syllogism in Logic merely is the method of obtaining consistent propositions.

Here again all the remarks made concerning material implication and Independence hold for material implication and consistency. In other words, the application of material implication to these problems brings into clear light the lack of generality of material implication. It prevents a set of propositions from being both consistent and independent, yet we have many sys-

tems which have both these properties. But it will be recalled that we defined total independence as propositions related as

where there is no overlapping, yet in order to have consistency there *must be* overlapping. Are we then to infer that totally independent propositions cannot be consistent?

It is necessary to distinguish two cases a) contradictory propositions b) non-contradictory propositions. If a) is true then obviously they cannot be consistent. In such a case total independence implies inconsistency. However, if the propositions are not contradictory, but merely refer to distinctly separated entities, such need not be the case. We use the concept of weakening, to define consistency for totally independent propositions.

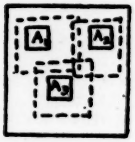
Let $A_1\ A_2 \ldots A_n$ be a set of propositions which are totally independent. Then if the propositions can be so weakened that they do have a common area, we call the set of totally independent propositions, *consistent with respect to total independence.*

For example, given a set of three propositions $A_1\ A_2\ A_3$ totally independent. This is illustrated by

Now if $A_1 \rightarrow \bar{A}_1$; $A_2 \rightarrow \bar{A}_2$; $A_3 \rightarrow \bar{A}_3$ where

$\bar{A}_1\ \bar{A}_2\ \bar{A}_3$ imply A_1
imply A_2
imply A_3

then if we denote the widened area with broken lines, we have



when we have consistency. If this is not possible (as in the case of contradictory propositions) we say the set is *inconsistent with respect to total independence.*

It is to be noted that in consistency, the logical product of any two propositions must not be irremediably zero. In independence we have two cases a) the product of any two is zero, but the product of one by the strengthened form of the other, or the product of the strengthened forms of both must not be zero b) the product of any one with the contradictory of the other must not be zero. We can express these conditions symbolically thus:

Let π stand for "logical product of"
A' stand for contradictory of A
$\bar{A}$ be the weakened form of A

a) Total Independence

$$\pi A_i A_j = o \ (i \neq j) \ (i, j = 1,2....n)$$

b) Partial Independence

$$\pi A_i A_j' \neq o \text{ (same conditions)}$$

c) Consistency with respect to total Independence

$$[\pi A_i A_j = o] \ [\pi A_i \bar{A}_j \neq o]$$

d) Ordinary consistency

$$\pi A_i A_j \neq o$$

For case c we can also have

$$[\pi A_i A_j = o] \ [\pi \bar{A}_i \bar{A}_j \neq o]$$

For cases b) and d) it is necessary to add

b′) $\pi A_1 A_2 \ldots\ldots A_j' \ldots\ldots A_n \neq o \quad (j = 1,2\ldots.n)$

d′) $\pi A_1 A_2 \ldots\ldots A_n \neq o$

For cases a) and c) cases similar to b′) and d′) are unnecessary, since to show the conditions to hold in pairs shows the condition to be true for the product of all.

If we interpret $= o$, as meaning "is impossible," then the cases show the existence or non-existence necessary for independence and consistency. As a consequence, we try to prove certain theorems.

Theorem 1. A set of propositions are consistent if there exists a range of objects satisfying them.

Proof. Suppose there was not. Then if $A_1 \ldots A_n$ is the set

$$A_1 \ldots A_n = o \text{ by definition of } = o$$

or

$$A_1 \ldots A_{n-1} \rightarrow A_n'$$

and therefore

$$A_1 \ldots A_{n-1}$$

cannot be consistent with A_n, because suppose it were, then by definition $A_1 \ldots A_{n-1} A_n \neq o$ for some values at least. But by hypothesis this is impossible.

Theorem 2. If a set of propositions have a range of objects satisfying them, they are consistent.

Proof. If there is a sphere it means that $A_1 A_2 \ldots A_n \neq o$ for some values at least, and therefore no subset will imply the contradictory of a proposition in the subset not containing the given subset for these values at least.

Therefore $A_1 A_2 \ldots A_n$ is consistent for these values.

The above two theorems constitute a necessary and sufficient condition for consistency. However, all our objections hold, and it is necessary as for independence to find a more or less general condition. If postulates are incompatible, no model can be found for them. Unless an impossibility proof is set up, the failure to find a model can give us no solution.

It is necessary, therefore, to prove two other theorems:

Theorem 3. If a set of propositions are consistent we can find a model in a finite number of steps.

This theorem is by no means evident, and may be impossible of proof when the so-called *Transfinite Axioms* are involved. If it can be proven then, we know that *if* we have a consistent set, we will eventually reach a model. We must then show

Theorem 4. If a set of propositions are inconsistent, we can show in a finite number of steps that a model is impossible.

The converse of theorem 3 is obviously true. The converse of theorem 4 is also obvious. It is the theorems as they stand which appear to be impossible of proof—particularly 4. And furthermore, if we throw 3 and 4 into negative forms as

Theorem 3'. If we cannot find a model in a finite number of steps, then the set is inconsistent.

Theorem 4'. If we cannot show in a finite number of steps that a model is impossible, then the set is consistent

we see that proving 3 and 4 will be extremely difficult. Theorem 4' is untrue because the failure to find a model in a finite number of steps, as was said before, proves nothing.

This immediately suggests a possible method of consistency proof by induction. This method consists in showing that the set of postulates is consistent for at least one value, then assuming it consistent for another value, to show that it must be consistent for the next. But if the set is consistent for *one* value, then we need proceed no further. It is a consistent set. No one claims, I believe, that in order for a set to be consistent it must be consistent for all possible values. Another variation of this method would be to show say two postulates to be consistent in a set of n postulates, then to assume $r < n$ consistent, and show that then $r + 1$ would be. It is, however, difficult to see how we could prove that if r propositions are consistent, that $r + 1$ would then be unless the $(r + 1)$ is always a theorem from the r. It does not follow that if one sub-set of a set of propositions is a consistent set, that the whole set is, any more than it follows that if one

subset of a set of numbers is ordered that the set is an ordered set. It would be necessary to show that *any* subset no matter how selected, is a consistent set. But if this can be shown, the induction is unnecessary, since it follows at once that the set is consistent.

As interesting as all these attempts are, it must be said that they are premature. Consistency depends for its solution upon the completion of logic, and upon a knowledge of the atoms of any given system whose consistency is to be shown. Meanwhile efforts should have been directed to discover the properties of consistency, in this way furthering, really, the advance of logic. Lewis and Langford have attempted to do this in "Symbolic Logic." We shall discuss this system elsewhere. Meanwhile we give an extension of his definition which we feel is necessary.

We shall use to express the fact that p and q are consistent;

Definition. $\Box\, pq = (p \rightarrow q')' + (p' \rightarrow q)'$

From which we get

$$- \Box\, pq :\equiv: [p \rightarrow q']\ [p' \rightarrow q] :\equiv: p = q'$$

We use the ordinary set of postulates for Logic.

$\Box\, pq$ is to be read *p is consistent with q*

$- \Box\, pq$ is to be read *p is not consistent with q*

$(p \rightarrow q)'$ is to be read *p does not imply q*

p' is to be read *p is false* or *not – p*

We set up our truth-value matrices.

p	q	$(p \rightarrow q')'$	$(p' \rightarrow q)'$	$\Box pq$
T	T	T	F	T
T	F	F	F	F
F	T	F	F	F
F	F	F	T	T

This shows that under our definition, two true propositions and two false propositions can alone be consistent, never a true and a false. This takes care of consistent lying.

Notice that according to our definition, the consistency of p and q means that either it is false that p and q together are impos-

sible or else that p′ and q′ together are impossible. If we consider the universe to be divided into 4 quadrants by our two propositions, then our definition affirms the existence of the two quadrants pq and p′q′. To affirm the existence of only one of the quadrants is not sufficient. We give below a further method of analysing the results that follow upon the assumption of the existence or non-existence of the various quadrants. Meanwhile we prove:

Theorem 1. $\Box p = (p \to o)' + (o \to p)'$

Proof. In definition let $q = 1$

$\Box p \cdot 1 = \Box p \equiv (p \to o)' + (p' \to 1)' \equiv (p \to o)' + (o \to p)'$

Theorem 2. $\Box pq = \Box qp$

Proof. $\Box pq = (p \to q')' + (p' \to q)'$

$= (q \to p')' + (q' \to p)'$

$= \Box qp$

Theorem 3. pq′ is the usual definition of Independence.

Proof. $\Box pq' \equiv (p \to q)' + (p' \to q')'$

$\equiv (p \to q)' + (q \to p)'$

i.e. p does not imply q or q does not imply p.

Now if it is possible to have two propositions both consistent and independent, the two relations $\Box pq$ and $\Box pq'$ must be possible for some values at least. By definition $\Box pq$ is not the contradictory of $\Box pq'$ (the contradictory being $- \Box pq$)

Theorem 4. $(\Box pq)(\Box pq') \gtrless o$

Proof. $\Box pq = (p \to q')' + (p' \to q)'$

$\Box pq' = (p \to q)' + (q \to p)$

$(\Box pq)(\Box pq') = (p \to q)'(p \to q')' + (p \to q)'(p' \to q)'$

$+ (q \to p)'(q \to p')' + (q \to p)'(q' \to p)'$

If any one of the terms is not zero, the sum cannot be.

Let us take $(p \to q)'(p \to q')'$ and rewrite it as $(pq' \to o)'$. $(pq \to o)'$ which we interpret as meaning pq′ does not vanish and also pq does not vanish.

Using a diagram we used before

p′	p	p′
q′	q	q′

which shows that both pq and pq′ exist. This clearly shows that p is consistent with q′ is not the same as p is inconsistent with q.

Theorem 5. $p \rightarrow \Box p$

Proof. $p(p = o) \rightarrow o$

$p = o \equiv (p \rightarrow o)(o \rightarrow p)$ Df.

$p(p \rightarrow o)(o \rightarrow p) \cdot \rightarrow \cdot o$

$\Box p = (p \rightarrow o)' + (o \rightarrow p)'$ by theorem 1

$\therefore - \Box p = (p \rightarrow o)(o \rightarrow p) \equiv (p = o)$

$p(- \Box p) \rightarrow o$

$p \rightarrow \Box p$

i.e. if p is true, it is self-consistent.

Theorem 6. $p \rightarrow p(- \Box - \Box p)$

Proof. $- \Box p \cdot \equiv \cdot p = o$ by definition

$- \Box - \Box p \equiv \Box (p = o)$

$= [p = o] \rightarrow o + o \rightarrow (p = o)$

$p[\Box - \Box p] = p[(p = o) \rightarrow o] + \cdot p[o \rightarrow (p = o)]$

Using now $p \rightarrow q \cdot \rightarrow \cdot p' + q$ we obtain

$$p[(p = o)' + o] + p[1 + (p = o)] = p(p = o)' + p(p = o) + p = p$$

$p \Box - \Box p \rightarrow p$

or

$$p \rightarrow p(- \Box - \Box p)$$

i.e. p is true implies that p is true and at the same time its inconsistency is inconsistent.

This is very much like the famous Brouwer axiom.

Theorem 7. $\Box (pq)r = \Box p(qr)$

Proof. $\Box (pq)r = (pq \rightarrow r')' + [(pq)' \rightarrow r]'$

$\Box p(qr) = [p \rightarrow (qr)']' + (p' \rightarrow qr)'$

We show the terms equal thus

$[p \rightarrow (qr)']' = (pqr \rightarrow o)' = (pq \rightarrow r')'$

$(p' \rightarrow qr) = (o \rightarrow pqr)' = [(pq)' \rightarrow r]'$

$\therefore \Box (pq)r = \Box p(qr)$

This theorem shows that if any *n* propositions are consistent, then any n-r combinations will be consistent with the other r.

We notice that in terms of the symbolism used by Dr. Smith our definition of Consistency can be expressed as follows:

$$\Box pq = |pq| + |p'q'|$$

which suggests a use of the expansion for $|pq|$ as given above.

Furthermore, it is to be noticed that consistency always can be interpreted in terms of the vanishing of certain quadrants. In the case of two quadrants, if $(p\ q')$ or $(q\ p')$ vanish, always, then the propositions are not independent, since then $p \rightarrow q$ in the other case $q \rightarrow p$. If the quadrants (pq) or $(p'q')$ vanish, then the propositions are inconsistent. There always remains the final case in which none of the quadrants vanish. Before offering a method of *quadrant analysis*, we remark that the process can always be generalized even though a very lengthy process may be involved, due to the complexity of the forms of the given propositions. As a result what holds of two propositions in this analysis may be extended into a general case. It can easily be seen for the case of n-propositions that if the segments representing n-1 and the contradictory of the nth vanish, that the propositions are not independent. If the segments representing the n propositions and the contradictory of the n propositions vanish, then the propositions are inconsistent. This may be due to the fact that some subset are inconsistent, but that we can obtain another subset which is consistent. It becomes necessary as a result, to discard propositions one by one until we get a consistent set. If we never do reach such a consistent subset, it may be because the propositions are totally independent, and it becomes necessary to weaken the propositions. If this cannot be done so that a consistent set is reached, then we are certain that the propositions are inconsistent.

We now offer one method of testing for quadrant analysis.[16] We explain only the cases of two propositions and three propositions, because on the basis of Theorem 7, we can continually reduce to these instances.

We have defined

a) $\quad |p| = (p \rightarrow o)'$ therefore we obtain

b) $\quad |pq| = (pq \rightarrow o)'$

[16] A great deal of what follows is due to Prof. H. B. Smith. It is advisable to consult his works before reading what follows. Cf. especially "Abstract Logic."

As special cases we obtain

a′) $|\,o\,| = o$
b′) $|\,1\,| = 1$

For convenience we rewrite the expansion formulas

c) $|\,pq\,| = pq + p'q'\,|\,p\,|\,|\,q\,| + |\,p\,|\,|\,q'\,|' + |\,q\,|\,|\,p'\,|'$
c′) $|\,pq\,|' = |\,p\,|' + |\,q\,|' + pq'\,|\,p'\,| + qp'\,|\,q'\,|$

We recall that c) and c′) fail whenever one of the quadrants vanishes. Thus when the formulas are applied and they break down, we are certain that one at least of the quadrants vanish.

The best way to explain the analysis is to take a concrete example. Therefore let

1) $p = (a \to b')' + (b' \to a)'\,\{(a \to b) + (b \to a)\}$
$q = (a \to b)' + (b \to a)'\,\{(a \to b') + (b' \to a)$

Conditions: 1) p and q are functions of the propositions a and b whose forms are given

2) $a \to b \equiv$ a implies b

Problem: To determine under what conditions on the propositions entering into the functions, the functions p and q are independent or consistent; i.e. what conditions on a and b will make the various quadrants of p and q vanish or remain.

We first re-write 1) and 2) in modal symbolism

1′) $p = |\,ab\,| + |\,b'a'\,|\,\{\,|\,ab'\,|' + |\,ba'\,|'\}$
2′) $q = |\,ab'\,| + '\,ba'\,|\,\{\,|\,ab\,|' + |\,b'a'\,|'\}$

We now assume ab = o (i.e. ab are inconsistent together) while the other quadrants exist. This is the following case.

a′	a	a′
b	b′	b

3) Then $p = |\,o\,| + |\,b'a'\,|\,\{\,|\,ab'\,|' + |\,ba'\,|'\}$

Since a′b′, ab′, and ba′ do not vanish we can apply the formulas c and c′).

$$|\,b'a'\,| = a'b' + |\,a'\,|\,|\,b\,|' + |\,b'\,|\cdot|\,a\,|'$$
$$|\,ab'\,|' = |\,a\,|' \text{ since } a \rightarrow b'$$
$$|\,ba'\,|' = |\,a'b\,|' = |\,b\,|' \text{ since } b \rightarrow a'$$

3') $$p = |\,a\,|'|\,b'\,| + |\,a'\,|\,|\,b\,|'$$

using as theorems: $- |\,o\,| = o$

$$a'|\,a\,|'b' \rightarrow |\,a\,|'\cdot b'$$
$$a\,|\,a\,|' = o \text{ and } o.x = o$$
$$b' + |\,b'\,| = |\,b'\,|$$
$$|\,a\,|'\cdot|\,a\,|' = |\,a\,|'$$
$$|\,a\,|'\cdot|\,a'\,| = o$$

3'') $$p = \{|\,a\,| + |\,b'\,|'\}\{|\,a'|' + |\,b\,|\}$$
$$= |\,a'\,|' + |\,a\,|\,|\,b\,| + |\,b'\,|'|\,a'\,|' + |\,b'\,|'$$

or $$p' = |\,a'\,|' + |\,a\,|\,|\,b\,| + |\,b'\,|'$$
$$q = |\,a\,| + |\,b\,|\{1 + |\,b'a'\,|'\}$$

4) $$q = |\,a\,| + |\,b\,|$$ using as theorems
$$|\,ab'\,| = |\,a\,|$$
$$|\,ba'\,| = |\,b\,|$$
$$1 + x = 1$$

4') $$q' = |\,a\,|'\,|\,b\,|'$$

We are now ready to analyze our quadrants:—

$$pq = \{|\,a\,|'\,|\,b\,|' + |\,a'\,|\,|\,b\,|'\}\{|\,a\,| + |\,b\,|\}$$
5) $$pq = |\,a\,|\,|\,a'\,|\,|\,b|' + |\,a|'\,|\,b'|\,|\,b| = o \text{ since } a \rightarrow b';\ b \rightarrow a'$$
$$p'q = \{|\,a'\,|' + |\,a\,|\,|\,b\,| + |\,b'\,|'\}\{|\,a\,| + |\,b\,|\}$$
6) $$p'q = \{|\,a'\,|' + |\,a\,|\,|\,b\,| + |\,b'\,|'\} = p'$$
$$pq' = \{|\,a\,|'\,|\,b'\,| + |\,a'\,|\,|\,b\,|'\}\,|\,a\,|'\cdot|\,b\,|'$$
7) $$pq' = |\,a\,|'\,|\,b\,|' + |\,a|'\,|\,b\,|' = |\,a\,|'\,|\,b|' = q'$$
8) $$p'q' = \{|\,a'\,|' + |\,a\,|\,|\,b\,| + |\,b'\,|'\}\,|\,a\,|'\cdot|\,b\,|' = o$$

Using as theorems for

5) $$|\,a\,|\,|\,a\,|' = o$$
6) $$|\,a\,|\cdot|\,a'\,|' = |\,a'\,|'$$
7) $$|\,b|\,'\cdot|\,b'\,| = |\,b\,|' \text{ and } x + x = x$$
8) $$|\,a\,|'\cdot|\,a'\,|' = o$$

We shall analyze now the case in which none of the quadrants vanish; namely

a′	a	a′
b		b′

For the case above, p and q were consistent and not independent, while p′ and q′ were inconsistent.

In this case our expansion formulas hold, and we must apply them.

9) $pq = [\,|\,ab\,|\,+\,|\,b'a'\,|\,\{\,|\,ab'\,|'\,+\,|\,ba'\,|'\}]$
$[\,|\,ab'\,|\,+\,|\,ba'\,|\,\{\,|\,ab\,|'\,+\,|\,b'a'\,|'$

which can easily be verified not to vanish since in order to vanish each term of the result must vanish and the product term of $|\,ab\,|\,|\,ab'\,|$ reduces to $|a'|'\,b\,|\,b'\,|$.

The remaining cases are merely matters of detail. They can easily be verified by actual multiplication in every case.

It is significant to note in conclusion that this method gives not only the data for pq and p′q′ but for all possible cases. In other words, the method tells much more than ordinary independence and consistency facts.

APPENDIX I

Consistency Proof for Converse of Brouwer's Axiom with Lewis' Postulate Set

In the appendix to Lewis and Langford's "Symbolic Logic" is given a proof of the consistency of the A set of axioms. Later in the appendix certain other axioms are introduced—in particular the Brouwer axiom, C 12. It is further stated that this axiom is consistent with the set A and the usual consistency proof is given.

That we can no longer place the usual faith in consistency proofs alone is evidenced by the following consistency proof of the A set with the *converse* of Brouwer's axiom. This would not be so interesting were it not for the fact that the Brouwer axiom is also independent of the other axioms. It was not thought necessary to prove independence of the converse because if it were not, then we should have a theorem

$p = \sim \Diamond \sim \Diamond p$ breaking down all distinctions between a strict and material system.

Our proof shows of course that

$p = \sim \Diamond \sim \Diamond p$ is also consistent with the other axioms.

What is needed is a proof of the generality of a set of axioms.

Consistency Proof

A 1. $pq \rightarrow qp$
A 2. $qp \rightarrow p$
A 3. $p \rightarrow pp$
A 4. $p(qr) \rightarrow q(pr)$
A 5. $p \rightarrow \sim (\sim p)$
A 6. $p \rightarrow q \cdot q \rightarrow r :\rightarrow : p \rightarrow r$
A 7. $\sim \Diamond p \rightarrow \sim p$
A 8. $p \rightarrow q \cdot \rightarrow \cdot \sim \Diamond q \rightarrow \sim \Diamond p$
A 9. $\sim \Diamond \sim \Diamond p \rightarrow p$ (this is the converse of the Brouwer axiom)

We use the matrix for pq and that for p as in the text, but alter that for p in one respect only.

pq	1	2	3	4
1	1	2	3	4
2	2	2	4	4
3	3	4	3	4
4	4	4	4	4

p	$\sim p$
1	4
2	3
3	2
4	1

p	$\Diamond p$
1	1
2	1
3	2
4	3

We define $p \rightarrow q$ as meaning p includes q; where any number includes itself and all numbers less than itself.

The main implication sign in 6 and 8 holds if and only if the inclusion on the right is true.

Proof. 1. Postulates 1–6 are consistent since they do not involve $\Diamond p$ where the alteration was made.

2. Postulate 7.
$\sim\Diamond p \rightarrow \sim p$

p	$\sim\Diamond p$	$\sim p$
1	4	4
2	4	3
3	3	2
4	2	1

3. Postulate 8.
$p \rightarrow q :\rightarrow: \sim\Diamond q \rightarrow \sim\Diamond p$

p	q	$\sim\Diamond q$	$\sim\Diamond p$
1	1	4	4
2	1	4	4
3	1	4	3
4	1	4	2
2	2	4	4
3	2	4	3
4	2	4	2
3	3	3	3
4	3	3	2
4	4	2	2

4. Postulate 9.
Brouwer-converse.
$\sim\Diamond\sim\Diamond p \rightarrow p$

$\sim\Diamond\sim\Diamond p$	p
2	1
2	2
3	3
4	4

That a consistency proof is in itself not sufficient can be shown by the following argument.

Let us assume we have a set of propositions $P_1, P_2 \ldots P_n$ which have been shown to be consistent. But let us further assume that p_n is a proposition obtained from $\bar{p}$ by mere specializing of the variables i.e. the propositions p_i ($i = 1 \ldots n - 1$) do *not* enter into the deduction of p_n. It is further assumed that $p_1, p_2, \ldots p_{n-1}$ are also consistent.

Now consider—having written down our set $p_1 \ldots p_n$, and not knowing that

$$p' \text{ implies } p_n$$

we can show that $p_1 \ldots p_n$ is a consistent system.

But notice that we will have only a special set of axioms and not a general one.

Independence proofs will not help us here because if p′ is independent

of the others p_n will also be, since it is obtained by mere specialization of the variables in p'.

This shows the fact that consistency alone is not sufficient for axiom sets and that a consistency proof does not in the last analysis prove very much. This is further supported by the fact that despite the proof of consistency for *Principia Mathematica*, Huntington's sixth set of postulates contains a postulate not in *Principia Mathematica* and independent of the axioms of *Principia Mathematica*. This fact has been brought out by B. A. Bernstein in a paper in the Bulletin of the American Mathematical Society for October 1933. (Huntington's sets of Postulates are in Trans. Am. Math. Soc. 35 pp 274–304.)

			p → ~ ◇ ~ ◇ p
p	~ ◇	p	~ ◇ ~ ◇ p
1		4	2
2		4	2
3		3	3
4		1	4

shows the Brouwer axiom is independent of the set.

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